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AN
INQUIRY
INTO THE
REMOTE CAUSE
OF
URINARY GRAVEL.

BY
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SOC. MED. EDIN. SOC.

EDINBURGH:
PRINTED FOR T. DUNCAN, N° 15. SOUTH-BRIDGE-STREET;
AND
J. JOHNSON, N° 72. ST. PAUL'S CHURCH-YARD,
LONDON.
M,DCC,XCII.

R53238

TO
DR. JOSEPH BLACK,
PROFESSOR OF CHEMISTRY IN THE
UNIVERSITY OF EDINBURGH,
ETC. ETC.

AND
DR. ALEXANDER MONRO,
PROFESSOR OF ANATOMY AND SURGERY
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ETC. ETC.

GENTLEMEN,

WHETHER I
look up to You as Instructors,
or consider You as Friends,
there are none, to whom, with
equal

equal propriety, I can dedicate this little Work. If it merit your Approbation, that will encourage the Diffidence I feel on laying a first Attempt before the Public, and amply repay any pains it has cost me. But, however little this Paper may deserve your regard; I hope You will look upon my offering it to You, as a Testimony of that Esteem from an Individual, which the
World

World have for your Characters ; and a Public Acknowledgment of the Attention which You have honoured me with, since I became your Pupil.

I AM, WITH THE GREATEST RESPECT,

G E N T L E M E N,

YOUR MOST OBEDIENT SERVANT,

ALEXANDER PHILIP WILSON.

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I N T R O-

INTRODUCTION.

THE attempt to remove the Proximate Cause of Urinary Gravel by Lithontriptics, has been attended with so little success ; and, at the same time, there is so uncertain a prospect of finding a safe and effectual solvent for the Urinary Calculus ; that the mind is naturally led to an investigation of the *remote cause of this disease* ; and to endeavour, by determining this, to lay down certain rules, which might tend
to

to prevent a complaint, which the present knowledge of medicine seems but ill fitted to cure. We attempt this with the greater prospect of success, as the experience of every age has told us, that there are certain circumstances, giving a predisposition to gravel, which no succeeding cure seems capable of counteracting.

But, although Experience, the best of guides, has pointed out many of the predisposing causes of Gravel; yet, unassisted by reason, it cannot investigate the change induced on the body, which constitutes the *remote cause of the disease*, and the hinge on which

which every rational plan of preventing it must turn. Ever in quest of lithontriptics, and often deluded by a deceitful appearance of what they fought for, physicians seem to have paid but little attention in order to determine the cause of this disease. Anxious to relieve the complaints occasioned by its attack, they have neglected ascertaining the means of preventing its first appearance. But, as this is surely the easier task of the two, let us endeavour to determine the manner in which the predisposing causes of gravel act; and see if, by doing so, we can lay down any rules for preventing this severe disease, which
has,

has, from the infancy of medicine, demanded the particular attention of physicians, and defied the most assiduous application of their art. It is with this view I have made the following experiments ; of the importance of any thing new I am to offer, experienced practitioners must judge ; my part is to assert, that I have been careful in marking appearances, and have drawn those conclusions only, which seem supported by fact.

The great connection which dyspepsia has with urinary gravel, both in giving the pathology of the disease, and in laying down indications for its
cure,

cure, has induced me to divide this paper into *two* parts; the latter of which treats solely of the morbid affections of the stomach, and is subdivided into three chapters, the first containing a short introduction, the second an inquiry into the proximate cause of dyspepsia, and the last, the treatment of that disease. The former part is subdivided into five chapters; the first, containing experiments made with a view to determine the remote cause of Urinary Gravel; the second, some remarks concerning the depositions of the Urine, and the manner in which acids act on that fluid; the third, general observations
on

on the foregoing Experiments ; the fourth, the application of the foregoing experiments to determine the remote cause of Gravel ; and the last, the circumstances rendering it probable, that the remote cause of Gravel is present ; and the means for removing it.

I proceed, then, to relate the Experiments ; making such remarks as may occur, but reserving the more general observations for the third chapter.

PART I.



CHAP. I.

*Experiments, made with a view to determine
the Remote Cause of Urinary Gravel.*

BEFORE I proceed to relate these experiments, it is necessary to take notice of the following circumstances.—Having been made at different times, they were not all performed with the same degree of accuracy. All of them, however, seem sufficiently accurate to enable us to determine with certainty concerning the result; and this is all

A

that

that is necessary. Besides, every point is proven by the most accurate experiments, and their result confirmed by others less so. The chief circumstance wherein the inaccuracy of these last experiments consists, is, that I did not take notice of the state of the thermometer, as they were performed ; but as hardly any of them lasted above three days, it is not probable that the temperature could vary much : and, as it must be evident, from considering the experiments where the state of the thermometer is mentioned, that even considerable changes in this did not vary the result ; it will appear, that every experiment I am to relate has been performed with all the accuracy requisite. Where the thermometer was used in any experiment, the state of it is generally mentioned every day till the urine was examined ; although, in relating the experiment, the deposition of the urine is always

men-

mentioned on the day on which it was made, that the result may be the more easily perceived. With regard to the state of the persons on whom these experiments were made, their particular situation at the time is afterwards mentioned. It is enough at present to observe, that none of them had ever been troubled with any calculous affection.

EXPERIMENT I.

IN this case, as well as in all the other experiments which I am to relate, I kept the dishes containing the urine, in a place at a distance from any fire, and consequently little subject to changes of temperature.—The thermometer, where it is mentioned, stood in the same place.

The following experiment I made on myself when in good health.

Living partly on animal and partly on vegetable food, and at the same time taking a certain degree of exercise ; morning, mid-day, and evening, I set apart a portion of urine in clean vessels. After these had stood 48 hours, I found in each of them a sandy deposition, which, after Sheele, I shall in the sequel call the *lithic acid* ; the several depositions, taken together, weighed gr. i. β .

On the following day, living in the same manner with respect to diet and exercise, I set apart, as formerly, portions of urine, which, after standing the same length of time, had also deposited gr. i. β . of lithic acid ; on the evening of this day I eat a lemon. Next morning I set apart a portion of urine, eating at this time a second lemon ; about dinner-time I eat a third, and
set

set apart another portion of urine; In the evening I eat another lemon, and set apart another portion of urine. All this day I had taken no animal product but milk, which is acefcent, and had taken nearly the fame quantity of fluid as on the former day, and the fame exercife. After each of the above portions of urine had flood 48 hours, I found in them a depofition of lithic acid, amounting to gr. iij. ſ. Next day I ufed the fame regimen; and the portions of urine, after ftanding the fame time, alfo depofited gr. iij. ſ. of lithic acid; the depofitions of the two laft days not differing one fifth of a grain.

The portions of urine fet apart on the different days of this experiment were exactly equal to one another; as were thofe in every experiment I am to mention; except where the whole urine made was kept.—

Either of these methods evidently gives a result sufficiently decisive. I also used, in all these experiments, vessels of a similar size, and similarly shaped ; for the vessel exposing a larger or less surface to the air, evidently affects the deposition.

One fact, which seems curious, I observed to take place in this experiment, and as often as I examined my urine, at different times of the day, except in particular circumstances afterwards to be mentioned ; the morning urine was darkest coloured, that made at mid-day next, and that in the evening palest.

EXPERIMENT II,

THIS was made on a boy about 15, in good health, which he enjoyed during all the experiments which I made on him.

Monday

Monday.

On getting out of bed, at 8 in the morning he made water ; this I did not keep. Having eat nothing this morning before 9, at that time he took for breakfast,

of bread,

of milk, *sing.* ʒ viij.

of honey, ʒ i.

At 11 in the forenoon, he set apart of urine ʒ vj. ; (Farenheit's thermometer standing at 29°.)

At 3 in the afternoon, he set apart of urine ʒ vj. (therm. 29°) ; at this time he eat of honey ʒ i. About half past 3, he took for dinner,

of four cream, ʒ xvj.

of bread, ʒ xj.

of sugar, ʒ vj.

At

At 6, he made of urine $\bar{3}$ iij. ; this I did not keep.

At 7, he took of bread, $\bar{3}$ vj.
of milk, $\bar{3}$ viij.

At half past 8, he set apart of urine $\bar{3}$ iij.
All his urine this day was $\bar{3}$ xjx, his drink being $\bar{3}$ xvj ; beside $\bar{3}$ xvj. of four cream, which was semifluid. All the three portions of this day's urine, after having been kept 48 hours, contained both crystals of lithic acid, and cream-coloured sediment ; the crystals of lithic-acid amounted to gr. $\frac{3}{4}$.

Tuesday.

On getting out of bed, at 8 o'clock in the morning, he made water ; this I did not keep.

At 9, he took for breakfast,
of bread, $\bar{3}$ viij.
of milk, $\bar{3}$ x. β .
of honey, $\bar{3}$ i.

At

At 11, he set apart of urine $\bar{3}$ vj. (ther. 32°)

At 3, he also set apart of urine $\bar{3}$ vj. (therm. 32°). At this time, he eat of honey $\bar{3}$ i.

At half past 3, he took for dinner,

of four cream, $\bar{3}$ xij.

of bread, $\bar{3}$ x.

of sugar, $\bar{3}$ vj.

At 6, he took of bread $\bar{3}$ vij.

of milk, $\bar{3}$ viij.

At half past 7, he set apart of urine $\bar{3}$ vij. (ther. 33°). All his drink this day was lb. i. $\bar{3}$ ii. beside the cream, his urine, lb. i. $\bar{3}$ v. I kept each portion of this day's urine 48 hours also. The appearances at the end of that time were as follow; in the morning urine I found a little cream-coloured sediment, with some lithic acid; in the mid-day urine no cream-coloured sediment, some lithic acid; the evening urine contained little cream-coloured sediment, and a good deal

deal of lithic acid. The whole lithic acid of this day's urine was gr. j. β .

Wednesday.

At 10 A. M., thermometer 33° ; at 3 P. M. thermometer 33° .

Thursday.

He made water on getting out of bed, as usual, which I did not keep.

He took for breakfast at half past 9,

of fish, \mathfrak{z} v. β .

of water, \mathfrak{z} vj.

At 11, he set apart of urine \mathfrak{z} viij. (ther. 33°)

At 3, he set apart of urine \mathfrak{z} viij. (ther. 33°)

He took for dinner,

of beef, lb. β .

He took nothing after dinner, and at 8 in the evening, he set apart of urine \mathfrak{z} viij. ;
this

this day his drink was but ℥ iv. ; his urine $\text{℔. i.} \text{℥ vij.}$; so great a check had perspiration suffered by that sickly disgust, of which he complained from living so much on animal food. After each portion of this day's urine had stood about 48 hours, I found in all much cream-coloured sediment; but in none of them any lithic acid.

Friday.

Getting up as usual at 8, he made water, which I did not keep. His breath this morning had a very sour smell, and even infected his bed-room with the same. He now expressed so great a disgust for animal food, that I could not prevail on him to continue the use of it alone. He therefore lived to-day in the following manner.

At

At 9 in the morning, he took for breakfast,
 of mutton, ℥ iv. ℔ .
 of potatoes, ℥ vj.
 of small beer, ℥ viij.

At 11, he made of urine ℥ vij. The state of the thermometer to-day was, by mistake, not attended to; but that of the weather was not sensibly different from what it had been the day before.

At 3, he set apart of urine ℥ vj. and took for dinner,

of mutton, ℥ vj.
 of potatoes,
 of small beer, *sing.* ℥ viij.

He eat nothing more this night, till the experiment was ended.

At 7, he set apart of urine ℥ vij. ; his drink to-day amounted to ℔. i. his urine to ℔. i. ℥ iv. ; the sourness of his breath went off towards evening. This day's urine

rine was poured off, each portion about 48 hours after it had been made. The appearances were these : In the morning urine I found some lithic acid, no cream-coloured sediment. This was evidently the effect of the great sourness present on Friday morning ; indicated, as I have already mentioned, by the breath. In the second portion of Friday's urine, I found some cream-coloured sediment, and some lithic acid ; in the last portion there was no lithic acid, but a good deal of the cream-coloured sediment. It is remarkable how quickly the urine is affected by sourness in the alimentary canal. The urine, which was made in the morning when the breath was very sour, deposited crystals of lithic acid, without any cream-coloured sediment ; the mid-day urine, made when the sourness was nearly gone off, deposited both lithic acid, and cream-coloured sediment ; while the

even-

evening urine, made after every symptom of sourness had been gone for some time, deposited the cream-coloured sediment, but no lithic acid. The sandy deposition of the two first portions of this day's urine amounted to gr. i. β . On each day of the above experiment, the exercise was equal, and taken at the same time of the day.

Saturday.

Thermometer at 11, A. M., 33° ; at 3, P. M. 33° ; at night, 35° .

Sunday.

In the morning, thermometer 36° ; in the evening, 37° .

EXPERIMENT III.

I MADE this experiment on the same boy.

Thursday.

He got out of bed at 8, and made water, which I did not keep.

At 9, he took for breakfast,

of beef, \bar{z} ij. \bar{z} ij.

of bread, \bar{z} i.

At 11, he set apart of urine \bar{z} iv. (thermo-
meter 38° .)

At 3, he set apart of urine \bar{z} vj. (ther. 38° .)

At half past 3, he took for dinner,

of mutton, \bar{z} vij.

of potatoes, \bar{z} vj.

of small beer, \bar{z} iv.

At

At half past 8, he made of urine \bar{z} ii. and took nothing to-day after dinner-time ; at night he complained of sickness, from having lived so much on animal food. After each portion of this day's urine had stood about 48 hours, in all of them there was cream-coloured sediment, by no crystals of lithic acid.

Friday.

He got up at 8, and made water, which I did not keep ; his breath to-day had a sour smell, and infected his bed-room with the same.

At 9, he took for breakfast,
 of mutton,
 of bread,
 of water, *ing.* \bar{z} iv.

At 11, he set apart of urine \bar{z} iv. (therm. 37°)

At 3, he set apart of urine \bar{z} v. (therm. 37°)

At

At half past 3, he took for dinner,

of mutton, $\frac{3}{4}$ iv.

of potatoes, $\frac{3}{4}$ vj.

of water, $\frac{3}{4}$ iv.

At half past 7, he set apart of urine $\frac{3}{4}$ vj ; the sourness of his breath went off towards the evening. After each portion of this day's urine had stood about 48 hours, I found in that made in the morning about gr. $\frac{3}{4}$ of lithic acid, but no cream-coloured sediment ; in that made at 3 o'clock, P. M. gr. j. of lithic acid, no cream-coloured sediment ; in the last, no lithic acid, but some cream-coloured sediment. The exercise taken on each day of this experiment was equal.

I found constantly in this boy, that after he had lived a day chiefly or entirely on animal food, although there were no symptoms of sourness that night ; yet by next

B

day

day these were always very evident ; this acidity producing the same effects on the urine, as acid *ingesta* do : This effect went off toward the evening, the acidity of the breath also going off. It appears, therefore, that the acidity was was some way or other produced in the night time, when the boy used a diet chiefly composed of animal food. The great check given perspiration by the sickly state which was induced by the use of such food, in a person accustomed to a very different manner of life, seems to have had a great share in producing these effects on the urine, as will afterwards appear.

EXPERIMENT IV.

WAS made on the same boy mentioned in the last.

First

First Day.

He got out of bed at 8 in the morning,
as usual, and made water, which I did not
keep. At 9, he took for breakfast,

of boiled beef, ℥ iiij. ℔ .

of water, ℥ iv. (ther. 44°).

At 11, he set apart of urine, ℥ viij.

At 3, he set apart of urine ℥ viij.

At half past 3, he took for dinner,

of boiled beef, ℥ v.

of water, ℥ iv.

At half past 8 in the evening, he set apart of
urine ℥ viij. (thermometer 46°). He eat no-
thing after dinner, complaining of sick-
ness and a great disgust for animal food.
His drink this day was only ℥ viij., while
his urine was ℔ i. ℥ viij.; so much in this
case also did his manner of living check per-

spiration. I examined each portion of this day's urine about 48 hours after it was made. In all there was cream-coloured sediment, but no crystals of lithic acid.

Second Day.

As usual, he got out of bed at 8, and made water, which I did not keep; he still complained of a great disgust for animal food, and some degree of nausea; his breath felt sour. I prevailed on him, however, to live for this day as he had done yesterday; he therefore took for breakfast,

of cold boiled beef, \bar{z} iij. β .

of water \bar{z} iv.

At 11, he set apart of urine \bar{z} vj. (ther. 46°)

At 3, he set apart of urine \bar{z} vj. (ther. 46°)

At half past 3, he took for dinner,

of cold boiled beef, \bar{z} ij.

of water, \bar{z} iv.

He

He took nothing after dinner, till past 8 in the evening, at which time he again set apart of urine ℥ vj. (therm. 46°). All his drink this day amounted to ℥ viij. ; his urine to ℔ j. ℥ ij. I poured off the three portions of this day's urine, each 48 hours after it had been made ; in the morning urine I found some crystals of lithic acid ; in the second portion I also found crystals of lithic acid ; and in the third a very few ; in none was there any cream-coloured sediment. The result of this part of the experiment is very striking. On the first day, when there was no acidity present, all the three portions of urine deposited the cream-coloured sediment, but not the least particle of lithic acid : on the second day, when there was much acidity present, the urine exhibited just the contrary appearances ; every portion of it containing crystals of lithic acid, but no cream-coloured sediment. The crystals of

lithic acid found in the three portions of this day's urine amounted to gr. j.

Third Day.

The thermometer stood this day at 47° .

Fourth Day.

Having got out of bed, and made water at 8 in the morning ;

At half past 8, he took

of lemon juice, \bar{z} j.

of sugar, \bar{z} β .

At 9, he took for breakfast,

of milk, \bar{z} iiij.

of bread, \bar{z} viij.

At half past 11, he set apart of urine \bar{z} iiij.

(therm. 46°), and took

of lemon-juice,

of sugar, *sing.* \bar{z} j.

At

At 3, he set apart of urine \bar{z} iij. (ther. 46° .)

As his urine had been so scanty, I made him
drink between 11 and 3 o'clock,

of water, \bar{z} viij.

At half past 3, he took for dinner,

of apple-dumpling, \bar{z} xij.

of sugar, \bar{z} vj.

Immediately after dinner he took,

of lemon-juice,

of sugar, *sing.* \bar{z} j.

At half past 6, he took,

of bread, \bar{z} vj. β .

of milk, \bar{z} viij.

At half past 8, he set apart of urine \bar{z} iij.
(thermometer 44°).

All the urine of this day amounted to \bar{z} ix.
his drink, including lemon-juice, to β i. \bar{z} xjv.
So much had the lemon-juice, and vegetable
diet increased the excretion by the skin, for
he had no stool this day. If we compare
the proportion the drink bears to the urine
on

on this day, to what it bore to that excretion on the two former days, we shall perceive a very striking difference indeed. I poured off each portion of this day's urine, 48 hours after it was made, and found in each a little lithic acid mixed with much cream-coloured sediment.

Fifth Day.

He lived exactly as yesterday, each meal consisting of exactly the same food; he also took the lemon-juice and sugar, as yesterday, and in the same quantity; at 11 o'clock A. M. he set apart of urine \bar{z} iij.; at 3, P. M. of urine \bar{z} iv.; and at half past 8, of urine \bar{z} v. His belly was still rather bound; (therm. in the morning 44° , in the evening 45°), his urine this day amounted to \bar{z} xij.; his drink, which was always water, (except where the contrary is mentioned),
being

being lb j. ʒ iij. ; his exercise was equal on each day of this experiment. I poured off each portion of this day's urine 48 hours after it was made, and found in all of them much cream coloured-sediment, but no crystals of lithic acid.

Sixth Day.

Thermometer during this day 45° .

The result of this experiment may appear at first sight singular ; but if we consider that the lemon-juice and vegetable diet acted here as powerful diaphoretics, and consequently that the acid passed by the skin (for it will afterwards be shown that this organ secretes an acid from the blood, even by insensible perspiration) we shall not find it contradict the result of any of the other experiments. I also found that a quantity
of

of apples produced the same effects on this boy, whose perspiration was naturally very vigorous. And on making him eat a considerable quantity of honey, (two ounces twice a-day) and at the same time live on vegetable food, he complained of acid eructations; and it was evident that this acid also acted in the same manner as the fruit had done; his urine being very scanty, when compared to the quantity of drink he took; containing much cream-coloured sediment, and few or no crystals of lithic acid.

This experiment clearly shows how little acedcent *ingesta* predispose to gravel, where the action of the skin is vigorous. And from this, as well as the two preceding experiments, and one still more decisive, afterwards to be mentioned, we learn, that no abstinence from such food takes off the disposition to that disease, when the action of the

the

the skin is much diminished. These facts have been overlooked by physicians ; I shall therefore soon endeavour to confirm them by repeated experiments, and show, in the sequel, of how much importance they are in giving the pathology of gravel. For a little, however, I return to the present subject, *b. e.* by further experiments to prove, that, *cæteris paribus*, *acescent ingesta* increase the deposition of lithic acid from the urine.

EXPERIMENT V.

THIS was also made on the same boy.

Monday.

He got up as usual at 8 in the morning, and made water, which I did not keep ; At 9, he took for breakfast,

of

of beef,

of potatoes,

of small beer, *sing.* $\frac{3}{4}$ iv.

At 11, he set apart of urine, $\frac{3}{4}$ iij. (ther. 39°).

At 3, he took for dinner,

of salt fish,

of potatoes,

of small beer, *sing.* $\frac{3}{4}$ iv.

At 6, he set apart of urine $\frac{3}{4}$ ix. (therm. 38°), having made none since 11, about 48 hours after they were made, I examined each portion of this day's urine, and found in both some cream-coloured sediment, but in neither any crystals of lithic acid.

Tuesday.

He lived this day in his ordinary manner, (*b. e.* eating animal food once a-day) that the effects of his diet on the former might go off.

Ther-

Thermometer this day 35° .

Wednesday.

He got up at 8, and made water, which I did not keep.

At 9, he took for breakfast,

of bread,

of milk, *sing.* ℥ viij.

At 11, he set apart of urine ℥ iv. (ther. 33°)

He took for dinner,

of apple dumpling, ℥ xvj.

of sugar, ℥ j.

At 6, he set apart of urine ℥ viij. having made none since 11, as on Monday; (ther. 32°) his exercise on each day of this experiment was equal. After each portion of this day's urine had stood about 48 hours, I found in both some cream-coloured sediment, and a deposition of lithic acid, which amounted, on the whole, to nearly gr. j.

Thursday.

[30]

Thursday.

Thermometer 32°.

Friday.

Thermometer 29°.

EXPERIMENT VI.

THIS experiment was made on a young man about 20 years of age, and in good health.

He breakfasted, dined, and supped entirely on vegetable matters and milk ; at breakfast time he eat a lemon ; at dinner-time another, and a third in the evening. At 6 o'clock in the evening, he set apart a certain portion of urine, and at 10, another ; after each

each had stood 24 hours, there was deposited from them of lithic acid gr. ij.

Next day he eat no lemons, and dined chiefly on animal food ; at the same times of the day he set apart the same quantities of urine. After each of these had stood 24 hours, neither had deposited any sand at all, so much had his manner of living on the first day increased the disposition of his urine to deposite the lithic acid.

His exercise was about equal on each day of this experiment.

EXPERIMENT VII.

THIS was made on the same person.

First

First Day.

He breakfasted on beef and bread. For dinner he eat of the flesh of a pig, beef, and bread. For supper, beef and bread. He set apart no urine this day.

Second Day.

He did not complain of his manner of living, nor was there any sourness produced, as had taken place in the boy, from a similar diet.

He took for breakfast, beef and bread; for dinner, of the flesh of a rabbit, beef, and bread.

Morning, mid-day, and evening he set apart of urine $\frac{3}{4}$ iv. (thermometer this day 39°). After each had stood about 48 hours there was in all, some cream-coloured sediment,

ment, in none of them any crystals of lithic acid.

Third Day.

Thermometer 39°.

Fourth Day.

Thermometer 35°.

Fifth Day.

Having eat a lemon last night, he lived this day entirely on vegetable matters ; except that at dinner-time he took some broth, in which flesh had been boiled : he also eat two lemons. He set apart no urine this day.

Sixth Day.

This day he lived as yesterday, eating 3 lemons, one in the morning, a second at mid-day, and a third in the evening; at which times also he set apart portions of urine, each as formerly, $\frac{3}{4}$ iv. (thermometer this day 39°). Having examined these portions of urine about 48 hours after they were made, I found in all of them crystals of lithic acid; which, put together, amounted to about gr. j. β . In none of them was there any cream-coloured sediment.

Seventh Day.

Thermometer 39° .

Eighth

Eighth Day.

Thermometer 39°.

His exercise was equal on each day of this experiment.

EXPERIMENT VIII.

First Day.

THIS experiment was made on myself, when in good health. Having made water at 8 o'clock on getting up, which I did not keep,

At 9, I took for breakfast,

of beef,

of water, *ſing.* lb. ʒ.

At 12, ſet apart of urine ʒ vj. (therm. 37°).

Took about this time of water ʒ iv.

At 3, took for dinner,

of fish, \bar{z} vij.

of beef, \bar{z} ij.

of water, \bar{z} vj.

At 5, set apart of urine \bar{z} iij. (therm. 37°).

This day my drink and urine were nearly equal; the depositions of this day's urine are mentioned with those of the next.

Second Day.

Having got up about 8 in the morning, I made water, which I did not keep.

At 9, I took for breakfast,

of milk and water, \bar{z} x.

of bread, lb. j.

At 11, I eat a lemon; and at 12 another.

At 1, I set apart of urine \bar{z} vj. (therm. 35°)

Took for dinner,

of milk,

of bread, *sing.* lb. j.

At

At 5, set apart of urine $\bar{3}$ iij.

The urine of this day was nearly equal to the quantity of liquid taken. Exercise on both days equal.

Two days afterwards, I examined all these portions of urine at the same time ; in those of the first day, although they had stood three days, I found only 5 crystals of lithic acid ; in the second day's urine which had stood two days, I found about 120 such crystals ; in neither day's urine was there any cream-coloured sediment.

The experiments which have now been related, are sufficient to show, that considerable changes in the manner of living, produce very evident changes in the state of the urine ; but these appear from more trifling changes of diet ; having repeatedly ob-

served, that a single meal or two, more or less acefcent than ufual, (provided it were fo to a confiderable degree) affects very fenfibly the ftate of the urine.

It is fufficient to relate the following inftances, which I have feen confirmed by a great number of others.

EXPERIMENT IX.

THIS was made on a young man aged 19 years, and in good health ; living partly on vegetable, and partly on animal food ; he fet apart about 2 o'clock in the afternoon, a portion of urine ; next day he breakfasted as on the foregoing, and took about the fame degree of exercife ; after breakfast he eat about lb. j. lb. of apples ; and at 2, as on
the

the preceding day, set apart the same quantity of urine. After each had stood about 72 hours, I found in the latter of lithic acid gr. ij., in the former of the same, gr. j.

EXPERIMENT X.

THIS was made on myself, when in good health.

I supped chiefly on bread and milk. Next morning I breakfasted on the same ; and after breakfast eat some acescent fruit. About 2 o'clock in the afternoon, I set apart a portion of urine, which, after standing 24 hours, had deposited of lithic acid gr. j.

Next night I supped entirely on beef and bread ; the following morning I breakfasted on the same ; and at 2 o'clock P. M. set apart
a por-

a portion of urine equal to what I had done the day before. After standing 24 hours, it had deposited no crystals of lithic acid.

Having related a sufficient number of experiments to prove that acidity of the *primæ viæ* (*cæteris paribus*) increases the tendency of the urine to deposit the lithic acid; I now proceed to show, that, by whatever means we increase the excretion by the skin, the quantity of lithic acid found in the urine is diminished, and sometimes totally abstracted. Before I proceed to this, however, I shall relate the following experiment, as it renders the result of those I am afterwards to mention more conclusive.

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EXPERIMENT XI.

THIS I have not made either to convince myself or others of a fact every one is ready to grant, viz. that diluents, *cæteris paribus*, will diminish the quantity of lithic acid found in any given portion of urine.

But, as it is necessary to take nothing on this subject for granted; I made the following experiment, which I have accidentally had frequent occasion to repeat with a similar result.

On the night before this experiment, I drank ℥. j. of milk, and another of water; next morning, on getting out of bed I set apart a certain portion of urine; from this time till between 2 and 3 o'clock in the afternoon,

I drank lb. j. of milk, and iij. of water, and at this time set apart a portion of urine. After each of these had stood 48 hours, the sediment of lithic acid found in them amounted to gr. $j\frac{1}{4}$. A night or two afterwards I drank lb. j. of milk, and next morning set apart a portion of urine equal to that set apart on the first day. From this time till between 2 and 3 o'clock in the afternoon, I drank lb. j. of milk, and lb. ss. of water, and at this time again set apart a portion of urine equal to that of the former day. After each had stood 48 hours, they contained a sediment of lithic acid, amounting to gr. $ij\frac{1}{4}$.

My exercise and diet were similar on each day of this experiment.

I meant now to have tried the effect of exciting the action of the kidney by diuretics,

tics, and for this purpose took a quantity of cream of tartar. But I soon found that I could draw no conclusion from such an experiment: for if the urine were not much increased, we could not be certain of the diuretic having taken effect; if it were, there would be too much watery liquor separated, to leave it possible to judge with any degree of certainty concerning the quantity of lithic acid it contained. But I am inclined to think, for reasons which will afterwards appear, that increasing the action of the kidney by diuretics, is a more probable method of preventing the formation of gravely concretions, than the most careful employment of diluents.

I now proceed to show, that diaphoretics lessen the deposition of lithic acid from the urine; and shall begin with *exercise*, which
is

is by all allowed to be a most powerful diaphoretic.

EXPERIMENT XII.

THE following experiment not only shows how much the deposition of lithic acid from the urine is increased by indolence ; but also that this matter may separate from the urine in considerable quantity, independent of all acid *ingesta*.

About a year and a half ago, I was attacked by rheumatism, chiefly confined to the right side of my head, and right shoulder, unaccompanied with any degree of pyrexia. This affection was so severe, and continued for so long a time, that it confined me to bed for near five weeks, during which time I lived on beef-tea, and calf-foot jelly ; any
other

other food aggravating the pain of the head to a great degree. Yet, during this confinement, my urine deposited much more lithic acid than when I had taken my usual exercise, and lived on much more acescent diet. Having kept about half a pound of it 24 hours, I found that it had deposited about gr. ij. of lithic acid. This I repeated at least half a dozen of times with a similar result. After I had recovered from my indisposition, and renewed my usual exercise; I found that the above quantity of urine, kept the same time, deposited about gr. $\frac{3}{4}$ of lithic acid, often less, and hardly ever more. This I also often repeated; nor indeed have ever found my urine deposite so great a quantity of lithic acid as gr. ij. from half a pound in 24 hours; while I was taking exercise, however acescent my diet was. This fact might perhaps be partly attributed to the kidney, during indolence, separating less watery

watery liquor, and hence more lithic acid in a given portion of urine. But that the appearance is not explained by such a supposition, is clear from this, that, with a view of determining the point, having taken much diluent liquor, I still found my urine depofite more lithic acid than when I was taking exercise and lefs diluent liquor. That we must attribute the appearance juft now mentioned to the check given perfpiration by indolence, will appear evident, from what is related in Experiment xiv.

These obfervations account for the following remark of Scheele's : (it is made in the laft number of his treatife on the calculus veficæ) “ It is remarkable, (fays he,) that
 “ the urine of the fick is more acid, and
 “ contains more animal earth than that of
 “ healthy perfons.”

The

The result of the experiment now related, is confirmed by the following.

EXPERIMENT XIII.

WHEN in good health, I repeated the last experiment, as follows. I purposely remained at home two days without exercise, and found that half a pound of urine made on the second day, and kept 24 hours, deposited near gr. ij. of lithic acid, *b. e.* above double the quantity it did, when I was taking exercise, and using a similar diet. Having had often occasion to be confined since I began these experiments, either by business or indisposition, I have seen the result of the two last confirmed a great number of times; so that I must look upon it as well ascertained, that, *cæteris paribus*, the quantity of lithic acid deposited by the urine, is inversely as the exercise taken.

Nor

Nor is this all ; for I have constantly observed that, continuing in indolence, my urine not only deposited more lithic acid than usual in the mean while, but continued to do so for some time after I had returned to exercise. This I particularly attended to, in the two cases mentioned ; in the latter of which, for two days after returning to exercise, my urine deposited more than its usual quantity of lithic acid, and in the former, for no less a time than upwards of two weeks. These appearances are much connected with the state of the stomach ; hence probably they are more remarkable in those whose stomachs are most readily affected by indolence : but there are also other causes acting here, which tend to produce the above mentioned change on the urine, and which will appear, I hope, fully explained in the sequel of this dissertation. I shall only at present remark, that this effect of indolence

dolence cannot be accounted for, by supposing that the weakness of the stomach produces much acid in the *primæ viæ*, (which has already been shown to increase the deposition of lithic acid from the urine), and hence a greater than ordinary quantity of lithic acid in the urine; for in Experiment xii. we have seen the same effect take place where little aliment was used, and that entirely animal, and where there was not the least symptom of acidity present.

EXPERIMENT XIV.

IN this, and the three following experiments, it is shown, that sudorifics or medicines promoting sensible perspiration, diminish the quantity of lithic acid found in the urine.

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In

In the illness I have already mentioned, (experiment xii.) I repeatedly found that half a pound of urine, when kept 24 hours, deposited about gr. $ij\frac{1}{4}$. of lithic acid, although, as was formerly observed, my diet was not in any degree acedcent. With a view of removing my complaint, I took \mathfrak{z} j. of Dover's powder. After I had submitted myself to the brisk operation of this sudorific for 12 hours, while the sweat still flowed copiously, I set apart lb. j. of urine, all I had made this day, which, after standing 24 hours, had deposited no lithic acid at all. I again examined my urine after the effects of the sudorific were over, and found, that it now deposited as much lithic acid as before I had taken the Dover's powder. The above change, therefore, on the urine, I could attribute only to the increased action of the skin. I had drunk, indeed, that day more than I used to do, but my urine was
not,

not more diluted ; for its quantity was not greater than usual, the superfluous moisture running off by the skin.

EXPERIMENT XV.

THIS I had an opportunity of making on a man aged 50, and who had at that time rheumatic pains in the joints of the lower extremities.

In the morning he took ʒj. and gr. vj. of Dover's powder. In about half an hour the sweat broke out, and continued to flow freely all day ; as he did not complain of thirst, I allowed him to take only his usual quantity of drink, which did not much exceed lb. j. He had now been in a profuse sweat from 9 in the morning till 6 in the evening, at which time he set apart ʒ vij. of urine, while the sweat still flowed copi-

ously. This I kept for 48 hours, without finding that it had deposited any lithic acid; I found in it, however, much cream-coloured sediment; his urine this day was less than half the usual quantity.

It was necessary to compare his urine in its natural state with this; next day, therefore, when he was going about, as usual (for his complaints did not confine him), at the same time of the day (6 o'clock, evening) he set apart a similar portion of urine; this I also kept for 48 hours, and found in it at the end of that time, a copious sediment of lithic acid, weighing above gr. iij. He had taken about the same quantity of liquid this day, as on the former, his urine being much more copious.

EXPERIMENT XVI.

ABOUT 8 months after I recovered from the above mentioned illness, I submitted myself to the following experiment, when in good health.

On an evening I took gr. ij. of Dover's powder. Next morning I took gr. vj. more; about half an hour after this, the sweat broke out, and continued as I took a little warm drink, till 7 o'clock in the evening. Between 3 and 4 in the afternoon, during the sweat, I set apart a portion of urine, and between 6 and 7, another, while the diaphoresis still continued. From morning till this time, I had drunk lb. ij. of milk and water; my urine being only lb. j. Each of these quantities of urine I examin-

ed, after it had stood 24 hours, and found, that the lithic acid which they contained, amounted only to gr. $\frac{1}{4}$.

Next day, I treated myself in the same way; except that I underwent no diaphoresis. I took no exercise, and drank about lb. ij. of milk and water: my urine this day did not exceed lb. j. during the time of the experiment, viz. from 3 in the morning till 7 in the evening. The small quantity of urine was probably owing to the great abstraction of moisture, which had taken place the day before; it was fully equal, however, to what I had made that day; my food also this day was similar to what it had been on the former. At the same times of the day, I set apart similar portions of urine; on examining them, each 24 hours after it had been made, I found in them a deposition of lithic acid, weighing gr. iij. or 12 times

times the other deposition. Notwithstanding the greater deposition from the urine of the last day, that of the first was darkest coloured.

EXPERIMENT XVII.

THIS was made on the man mentioned in Experiment xv. ; after he was recovered from his late complaints, and going about his usual occupation as a day-labourer ; only troubled with two or three biles, a topical affection, and consequently of no importance in influencing our conclusions from the following experiment.

On Sunday morning, he took ʒj. of Dover's powder After he had sweated 5 hours, he set apart a certain portion of urine. From morning till this time, he had drank rather
more

more than usual ; but his urine was hardly equal to what he generally made.

On Tuesday, when going about his usual business, he set apart, at the same time of the day, another portion of urine. Both of these I examined on Wednesday morning. Now the whole urine made on Sunday, was hardly equal to that made on Tuesday ; but the quantity set apart on Sunday was greater than that set apart on Tuesday. He had taken no exercise on the former day, on the latter he had employed this liberally. The urine set apart on Tuesday had not stood 24 hours ; that set apart on Sunday, nearly 3 days. Yet in this I found only 21 particles of lithic acid, a quantity hardly sufficient to turn a nice balance ; while, in the Tuesday's urine, there was a deposition of lithic acid, weighing gr. j. β ., a dozen or more times the other deposition.

THE

THE cases now related shew, in the most unequivocal manner, that, by producing sweat, we diminish the quantity of lithic acid found in the urine. Although these are the only cases of this kind I have notes of; yet I have seen the same effect produced by sudorifics at other times, having never examined the urine, during their operations, without observing it.

The following experiments were made with *diaphoretics*, or medicines increasing insensible perspiration only.

EXPERIMENT XVIII.

THE person mentioned in Experiment ix. underwent the following; when in good health.

He

He took in small doses, from 12 o'clock noon, till 6 in the evening, gr. j. β . of tartar emetic. At 10, in the same evening he set apart a portion of urine. This stood 24 hours without depositing any lithic acid.

Next night, at the same time, he set apart an equal portion of urine ; after it had stood only 12 hours, I found in it a sediment of lithic acid.

He used similar diet and exercise, on each day of this experiment.

EXPERIMENT XIX.

THIS was made on the boy, whom I have already frequently mentioned, still in good health.

Having

Having examined his urine in its ordinary state; I found too little lithic acid deposited from it to enable me to draw any certain conclusions, even from its total abstraction; I therefore performed this experiment on him in the following manner.

When living in his usual way, except that he took rather less exercise, I made him eat four apples after breakfast; and, about 2 o'clock P. M. set apart a portion of urine; after this had stood 24 hours, it had deposited gr. j. of lithic acid.

Next day, he lived exactly as on this, except that he took from morning till noon, in small doses, gr. j. of tartar emetic; which produced no sensible effect. About 2, P. M. he again set apart a portion of urine, equal to what he had set apart the day before; after

after this had stood 24 hours, I found it had deposited no lithic acid at all.

EXPERIMENT XX.

THE following experiment was made on myself, while in good health.

Living as usual, but taking rather less exercise; I set apart, about 1 o'clock, p. m. a portion of urine, which, after standing 24 hours, had deposited a little more than gr. ij. of lithic acid.

Next day I took, in small doses, from morning till mid-day, gr. j. of tartar emetic; this occasioned such a degree of nausea, that I felt a cold sweat on the forehead; but there was no sensible perspiration on any other part
of

of my body. On this day I took no exercise, that the result of the experiment might be the more striking; living in every other respect as on the former day, the liquid I took being equal, but my urine less. On this day also, I set apart a portion of urine at 1 o'clock P.M. equal to what I had the day before. After this had stood 24 hours, I found that it had deposited only two or three particles of lithic acid, a quantity hardly visible, had it not been collected in one part of the vessel, and not to be measured by the nicest balance.

EXPERIMENT XXI.

LIVING as usual, but taking rather less exercise, I set apart about mid-day a portion of urine, which, after standing 24 hours, had deposited gr. ij. of lithic acid.

Next

Next day I took a smaller quantity of tartar emetic than I had done in the last experiment, not much above half a grain, which hardly produced any nausea : and lived in every other respect as on the first day of this experiment. About mid-day, I set apart a portion of urine equal to what I had the day before ; which, after standing 24 hours, had deposited no lithic acid at all.

On considering the four last experiments, it occurs, that, by increasing insensible perspiration, we have a more convenient method of preventing the urine from depositing its lithic acid, and not only this, but a more efficacious one ; for I have always observed, that a small dose of tartar emetic more certainly prevents the deposition of lithic acid, than a large one of Dover's powder, producing a copious sweat. This I would account for on the following principles : It is shown,
in

in the 3d chapter, from repeated experience that the secretion of the matter occasioning the deposition of lithic acid from the urine, depends not upon the mere relaxation of the kidney, but upon its vigorous action. I should imagine then, that the same thing takes place in the skin, and that this matter is only separated by it, in proportion to its activity. (For it will afterwards clearly appear, that the matter occasioning the deposition of lithic acid from the urine, passes also by the skin; and indeed, from the experiments already related, we can hardly suppose otherwise.) Now Dover's powder, although it may in some degree increase the action of the skin, yet we must suppose that its sudorific effect is in a great measure to be attributed to the relaxation induced on that organ, by the opium it contains; whereas antimony acts only by promoting the activity of the skin; and hence seems to proceed the property

perty it has, of throwing off the noxious matter in so remarkable a degree. There is also another difference in the manner in which these medicines affect the urine. While the Dover's powder produced in general no effect on the urine after the sweat had ceased to flow; the antimony continued, for several days after it was taken, in a greater or less degree, to affect that excretion. There is still another fact with regard to antimony, which I have repeatedly observed, viz. that the deposition of lithic acid from the urine was not so effectually prevented by this medicine, when it produced nausea, as when it produced no sensible effect on the body. I would account for this on the principles above mentioned; for although nausea produces sweat, this is evidently owing to the relaxation it induces on the skin; and a little reflexion will show, from the intimate connexion between the skin and stomach,

that

that we cannot suppose the one in a state of vigorous action, while the other is affected in a contrary manner; for nausea never tends to increase the action of the stomach, but evidently to diminish it; and indeed it affects in the same manner every function of the body, vital, natural, and animal.

Of all the medicines physicians are acquainted with, there is none which more uniformly and effectually supports the excretions than mercury. By proper treatment, we can generally direct its operation to the skin; in which circumstances, it proves a safe and powerful diaphoretic. On this account, I wished to try its effects on the urine; which I had an opportunity of doing, in the following manner.

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EXPERIMENT XXII.

A YOUNG gentleman of my acquaintance contracted a slight *sypilitic* affection, for which he was obliged to have recourse to mercury. He was a very proper subject for my observing the changes induced on the urine by this medicine, as I had had occasion to examine his urine in all situations, when he was in health ; at least 50 or 60 different times ; so that I knew perfectly what changes to expect from different manners of life. I found the state of his urine at all times much affected by indolence ; half a pound, when he remained at home, depositing about gr. ij. of lithic acid in 24 hours, although his diet was not more acefcent than usual.

When

When rubbing in 3 j. of mercury each day, staying at home without exercise, and living on vegetables alone, he set apart 3 iv. of urine ; this I kept 48 hours, and found in it no lithic acid, but a considerable quantity of cream-coloured sediment. At this time his urine was less than usual. From treating his affection carelessly, although at first a very slight one, he found it necessary to continue the use of mercury for no less a time than three months, during the whole of which time I carefully examined the state of his urine ; and constantly found, that, when it was much lessened in quantity, it deposited no lithic acid, but much cream-coloured sediment. For the first week or two, his urine was not above the half of its usual quantity ; as his stomach, mouth, and general health, however, became affected by the mercury, the above appearance began to go off : hence it is evident, that the mer-

cury acted at first as a diaphoretic ; this effect ceasing as the debility of the system increased ; and particularly that of the stomach ; the vigorous action of which is ever necessary for promoting the activity of the skin.

These appearances I saw take place a second time. He was persuaded to give up the mercury for a little, and try the effects of a pretty full diet. He became better in his general health ; (for the affection under which he laboured was too trifling to affect this,) and, on returning to the use of mercury, the same scanty urine took place, together with the same deficiency of the lithic acid, and increase of the cream-coloured sediment in that fluid. These effects, however, were neither so great nor permanent at this time as formerly ; his stomach too, and general health became sooner affected.

After

After he had remained at home for about three weeks, the mercury seemed to act as a diuretic. The urine was then of a lighter colour, in a greater than usual quantity, and deposited less lithic acid; undoubtedly owing to the greater proportion of watery liquor present, as more than usual ran off by the kidneys.

Before I leave this case, it is necessary to remark, that when the urine was so scanty, the belly was rather bound than otherwise. I also found in the person now mentioned, that applying mercury in the form of ointment to the skin, tended more to promote perspiration, than mercury taken by the mouth; although this did not produce the least cathartic effect; a circumstance probably owing to the mercury taken in this way, producing a greater degree of dyspepsia; which, in this patient, it always did.

Such were the appearances repeatedly observed in the case now mentioned. The effects of mercury on the urine, when acting as a diaphoretic, are also seen in the following experiment.

EXPERIMENT XXIII.

THIS was made on the boy whom I have frequently had occasion to mention, still in good health.

First Day.

On getting out of bed, at 8 o'clock in the morning, he made water, which I did not keep.

At 9, he took for breakfast,
of bread,

of

of milk, *ſing.* ʒ viij.

of honey, ʒ j.

At 11 in the forenoon, he ſet apart of urine
ʒ vj. (thermometer 39°).

At 3, he ſet apart of urine ʒ viij. (thermo-
meter 39°).

At this time, he eat of honey, ʒ j.

At half paſt 3, he took for dinner,

of ſour cream, lb. j.

of bread, ʒ xj.

of ſugar, ʒ vj.

At 6, he made of urine ʒ iiij. ; this I did not
keep.

At 7, he took,

of bread, ʒ vj.

of milk, ʒ viij.

He ſet apart at half paſt 8, of urine ʒ iiij.

All his urine this day was ʒ xjx. his drink
lb. j., and lb. j. of ſour cream which was
ſemiſfluid. I poured off each portion of this
day's urine, after it had ſtood 48 hours, and
found

found in all some cream-coloured sediment,
and some lithic acid ; this last amounted to
gr. $\frac{3}{4}$.

Second Day.

He made water, on getting out of bed, at
8, as yesterday ; this I did not keep.

At 9, he took for breakfast,
of bread, ℥ x. ℞.
of milk, ℥ viij.
of honey, ℥ j.

At 11, he set apart of urine ℥ vj. (thermo-
meter 32°).

At 3, he set apart of urine ℥ vj. (thermome-
ter 33°).

At this time, he took of honey ℥ j.

At half past 3, he took for dinner,
of four cream, ℥ xij.
of bread, ℥ x.
of sugar, ℥ j.

At

At 6, he took,

of bread, ʒ viij.

of milk, ʒ viij.

At half past 7, he set apart of urine ʒ viij.
(thermometer 33°).

He took no more to-day of any thing. His urine this day was ʒ xjx.; his drink lb. j. beside the cream. After each portion of this day's urine had stood 48 hours, I found in all of them a little cream-coloured sediment, with a considerable quantity of lithic acid, amounting to gr. j, β .

Third Day.

Thermometer this day 33° .

Fourth Day.

On the evening of this day, he took a mercurial pill; which he continued to do morning and evening, for four days.

Fifth

Fifth Day.

This was the morning of the fifth day since he began to take the mercury, which he continued to do to the end of the experiment.

He got out of bed, as usual, and made water at 8 o'clock in the morning, which I did not keep.

At 9, he took for breakfast,

of bread,

of milk, *sing.* ʒ viij.

of honey, ʒ j.

At 11, he set apart of urine ʒ v. (thermometer 38°).

At 3, he set apart of urine ʒ v. (thermometer 38°).

His mouth to-day was no affected.

At

At 3, he eat of honey ℥ j. ;

At half past 3, he took for dinner,

of apple-dumpling, lb. j. ℥ v.

of sugar, ℥ j.

At half past 5, he made of urine ℥ iij. ; this I did not keep. At this time he had one natural stool.

At 6, he took of bread, ℥ iv.

of milk, ℥ viij.

At 8, he set apart of urine ℥ ij. his whole drink this day amounted to lb. j. not including a considerable quantity of moisture in the dumpling, his urine to ℥ xv. ; he took nothing more to night but ℥ iv. of milk. After each portion of this day's urine had stood 48 hours, I found in all of them cream-coloured sediment ; but in none was there any lithic acid.

Sixth

Sixth Day.

His mouth to-day was a little affected,
but there was no sensible salivation.

He got up this morning as usual, making
water at 8, which I did not keep.

At 9, he took for breakfast,

of bread,

of milk, *sing.* ʒ viij.

of honey, ʒ j.

At 11, he set apart of urine ʒ iiij.

At 3, he set apart of urine ʒ vj. (therm. 36°)

At this time he took of honey ʒ j.

At half past 3, he took for dinner,

of apple-dumpling, lb. j.

of sugar, ʒ j.

At 6 in the evening, he took

of bread, ʒ vj.

of milk, ʒ viij.

of water, ʒ iv.

At

At 8, he set apart of urine ℥ iv. (therm. 35°)
He had one natural stool to-day.

All his drink this day was ℥ xx. beside the moisture in the dumpling; his urine only ℥ xij. I examined each portion of this day's urine 48 hours after it had been made; in the morning urine there was some lithic acid; and a small quantity of cream-coloured sediment: the lithic acid in this amounted to gr. β . In the other two portions there was no lithic acid, but some cream-coloured sediment.

Seventh Day.

Thermometer in the morning, 35° ; in the evening, 34° .

The experiment now related, would have been even more conclusive on a person whose
perspiration

perspiration was not so easily promoted ; for in this boy the vegetable diet alone was sufficient to produce this effect in a remarkable degree.

It is necessary that I should now say something of the excretion by the skin. M. Bertholet found that sweat contained an acid ; and there are many reasons which would incline us to believe that an acid also passes by insensible perspiration ; that this supposition is well founded, appears from the following experiment.

EXPERIMENT XXIV.

I TIED a piece of paper stained with litmus about my neck. After it had remained there 8 hours, during which time there was

no

no sensible perspiration on any part of my body, I found it changed to a red colour. This I again repeated; allowing the paper to remain applied only about 4 hours; and after that time found it also changed to a red. In making this experiment, I preserved a piece of the stained paper torn from what was applied, that by comparing the two pieces at the end of the experiment, the result might be the more decisive.

Having now finished the narration of these experiments, and the more particular observations which I intended making in this chapter, I proceed to what was proposed as the subject of the following.

C H A P. II.



*Some Observations concerning the Depositions
of the Urine, and the Manner in which
Acids act on that Fluid.*

THERE are still some circumstances to be ascertained, in order to set the result of the foregoing experiments in a clearer point of view. I mean the manner in which acids act on the urine, after it is out of the body; and the nature of those depositions, which I have had so frequently occasion to mention.

On the first of these heads I learned the following curious fact from an anonymous author.

author, after the treatise I am now laying before the public, was nearly completed: This author mentions, that on adding any acid, even the carbonaceous, to urine, he always procured a copious deposition of what he calls the concreting acid, which is the same matter I have mentioned under the name of the lithic acid.

This experiment I have repeated frequently, both with recent urine, and that which had been kept some time; using the vitriolic, nitrous, muriatic, acetous acids, or the acid of lemons; and in all instances found the result as the above mentioned author has stated it. On this subject I can add the following particulars, from repeated experiments.

Urine left to itself, deposites either a whitish matter, rendering it muddy, and this of-

ten, in an hour or two after the urine is made; or crystals of lithic acid; or sometimes both. From the number of experiments I have had occasion to make on this subject, I have had it in my power to observe, in the clearest manner, the different circumstances of life which produced the one or other deposition. The observations I am now to mention, are supported by all the experiments I have related, where the cream-coloured sediment is taken notice of; the reason why it is not always so is, that, when I first began these experiments, I neglected it, as an accidental appearance; and it was not till it had very frequently occurred, that I paid particular attention to it, in order to discover by what laws its appearance in the urine was regulated.

In the *first* place, then, with regard to these two depositions, I never found both
existing

existing in any considerable quantity in the same urine, but always observed, that where there was much of either, there was little or none of the other; from this, I was led to suppose, that the presence of the one was to a certain degree incompatible with that of the other. This opinion is confirmed by what I am soon to mention. *Secondly*, While the lithic acid was found in greatest quantity in the urine of a person living on an acefcent diet; the cream-coloured deposition was increased by food of a contrary tendency. *Thirdly*, Any cause encreasing perspiration, while it diminishes the quantity of lithic acid, tends to produce the cream-coloured sediment in the urine. *Fourthly*, The cream-coloured sediment is more soluble in the urine than the lithic acid. *Lastly*, The lithic acid is less easily acted on by acids than the cream-coloured sediment.

From these circumstances, as well as other considerations, we infer, that it is the lithic acid which is apt to form concretions in the urinary passages ; since it is less soluble, and more apt to concreate than the other, and is produced by that manner of life which experience has taught us, is apt to induce calculous complaints. We must also infer from them, that the secretion of any acid matter by the kidneys, tends to produce a deposition of lithic acid ; and at the same time to prevent that which, from its appearance, I have called the cream-coloured sediment : this I confirmed by experiment ; for, I always found, that the addition of an acid to the urine, while it produced a deposition of the lithic acid, prevented the appearance of the cream-coloured sediment ; and that, on adding it to urine which contained the cream-coloured sediment, but no crystals of
lithic

lithic acid, the former, sooner or later, disappeared, while the other was deposited, leaving the urine, formerly turbid with the cream-coloured sediment, perfectly transparent; nor is this an effect which will take place merely by keeping the urine for some time at rest; for, after keeping it for months, without the addition of an acid, it is always found as turbid as at first.

Another effect of acids on the urine is, that of changing its colour, which they reddened very considerably, and render darker: these effects appearing more suddenly if heat be applied *.

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There

* Vinegar and lemon-juice produce the precipitation of the lithic acid, without altering the colour of the urine. It also happens, that if the colour of the urine be much darkened by any acid, that of the crystals of lithic acid produced, is so likewise.

There are particular acids which also produce other phenomena, when mixed with the urine. The strong nitrous acid produces a violent effervescence with it, whether recent or not. There is, during this, a permanently elastic fluid disengaged, which precipitates the calcareous earth of lime-water, and produces no contraction on the admixture of atmospheric air; the diluted nitrous acid hardly produces any effervescence. The vitriolic acid produces the same effect on the urine, but in a less degree. The effervescence, in both cases, is much increased on the application of heat.

The muriatic acid excites no effervescence with urine, whether applied in its common, or oxygenated state: if urine be exposed to the vapours arising from muriatic acid, and calx of manganese, these are totally absorbed, but not the least elastic fluid is produced

duced from the urine, nor is any effervescence observed; neither do these appearances take place on adding to the urine the acetic acid, or the acid of lemons, although a considerable heat be applied.

The carbonaceous acid produces the same effect on the urine, which acids in general do. If urine be exposed for some time to the elastic fluid, produced on mixing chalk and vitriolic acid, its colour appears somewhat reddened, and the lithic acid is separated in a greater than usual quantity; but these effects are much less perceptible with this than any other acid, except that lemon-juice and vinegar seem to change the colour, in a still less degree.

I now proceed to say something of the depositions which spontaneously take place from the urine. The nature of one of these is pretty well understood; the other, viz. the cream-

cream-coloured sediment seems to have been entirely overlooked.

From considering M. Scheele's* experiments, and what I have now said concerning this substance, we would be inclined to think it the calcareous phosphat. It very evidently differs, however, from this salt; it never falls to the bottom of the vessel, leaving the urine limpid; which the calcareous phosphat always does. This last disappears immediately on the addition of an acid; the other very gradually, and continues to render the urine turbid for several days, or even weeks, after the acid has been added. The cream-coloured sediment is easily re-dissolved by the application of heat: the calcareous phosphat never is. We cannot, therefore,

suppose

* Mr. Scheele shews, that by adding volatile alkali to urine, we produce a precipitation of calcareous phosphat, owing to the neutralization of the superabundant phosphoric acid, which rendered this salt soluble in the urine.

suppose it calcareous phosphat. The following observations seem, with sufficient certainty, to point out its nature.

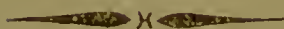
On adding a certain proportion of acid to urine, containing much cream-coloured sediment, and an equal quantity of the same acid to urine, containing little or none of it : I always found most lithic acid precipitated from that which had contained most cream-coloured sediment ; and likewise, that the more of this it contained, the longer time it required to become limpid ; and, for the complete deposition of the lithic acid to take place. Besides, where there was a long time required for the deposition of the lithic acid, which always happened as I have mentioned, when much cream-coloured sediment was present, one could easily perceive the gradual change induced on this last, which altered its colour, and being at the same time
 preci-

precipitated from the urine, was, by eegrees, totally changed into a dark red-coloured sandy-looking matter, lying at the bottom of the vessel.

These observations point out, that the cream-coloured sediment is the neutral salt containing the lithic acid ; from which it may be precipitated by perhaps every other acid ; which forming a new compound more soluble than the cream-coloured sediment ; the urine appears transparent, while the lithic acid is deposited in the form of very fine sand.

Having finished what I proposed to do in this chapter, I now proceed to make such observations, as immediately result from the experiments which have been related.

C H A P. III.



Some General Remarks on the foregoing Experiments.

ON taking a view of the experiments related in chapter ii. it appears, that a diet composed of animal food, or any cause promoting perspiration, lessens the tendency of the urine to deposite the lithic acid.

It does not appear difficult to explain this on the principles already laid down; we have seen every acid which was mixed with the urine, producing a precipitation of lithic acid: hence we must infer, when we see more than ordinary of this acid in the urine,

rine, on using acedent diet, that the acid derived from such diet, acts in a similar manner, thus producing that great quantity of red sediment we observe on such occasions. But however acid the diet may be, if we artificially increase perspiration, or if this be naturally very vigorous; the acid will pass by the skin, (for it has been shown, that an acid passes even by insensible perspiration), and hence produce none of its effects on the urine.

Here a very important question occurs; Does the body, by its own powers, generate an acid capable of precipitating the lithic acid from the urine? Or is such an acid always derived from acedent diet? Several of the above experiments seem to show, that this acid is constantly generated in the body, independent of all acid derived from the alimentary canal; and that from circumstan-

ces already taken notice of, it may pass in great quantity by the kidney, while the person uses aliment which can produce no acidity. We have seen the urine depositing much lithic acid, when there was little food taken ; and that which was entirely animal, continued not for a day or two, but several weeks ; and what plainly indicates, that it is an acid which acts in this as in other cases, is, that the increase of perspiration by a sudorific, prevented the deposition of lithic acid from the urine. See experiment xiv.

If we consider the different appearances of the urine, we shall find three distinct states in which this fluid exists at different times ; indicating different conditions of its secreting organs.

The first is, when the vessels of the kidney are constricted ; in this case, the urine
flows

flows limpid, and deposites little sediment of any kind; we see this taking place in the cold stage of fevers, from the application of cold to the surface of the body, &c. The second is, when the urine is as high coloured as usual, but deposites less lithic acid; the kidney seems now in a state of relaxation, rather than of vigorous action: this I infer, from having always observed the urine secreted during sleep, however short a time retained in the bladder, fully as high coloured as that secreted during vigilance, when every part of the system is in greater activity; this urine seems more frequently to contain the cream-coloured sediment, than that secreted when the kidney is most active. When the vigorous action of the kidney takes place, it forms the third state; here the colour of the urine is not higher than where mere relaxation takes place; it, however, deposites more of the lithic acid. This state
of

of the kidney is always induced by the following circumstances,

In the *first* place, by vigilance, as I have already mentioned ; *Secondly*, By any cause obstructing perspiration.

The skin and kidneys separate the same acid matters from the blood ; hence, when the action of the one is diminished, that of the other must be increased, in order to prevent an accumulation of acid in the system : hence it is, that the proper action of the skin being prevented, more of this acid passes by the kidney, and consequently there is produced in the urine a greater deposition of lithic acid. Whether this action of the kidneys may be produced by diuretics, and the system freed from any over proportion of that noxious matter, is a question I cannot positively answer, for the reasons given in

Experi-

Experiment xi. But, if we consider what has just been said, and for a moment reflect on the general laws of the animal œconomy, we must suppose, that, increasing the action of the kidneys by diuretics, is a manner better calculated for freeing the system of this acid matter, than the use of fluids acting merely as diluents, and which seem to be of little service, but as they wash out any particles of sand adhering to the kidney ; and as by increasing the proportion of fluid, they render the lithic acid rather less apt to be deposited : for Scheele and Bergman have shown that this matter, though difficultly, is soluble in watery liquors.

The three states in which the kidney exists at different times, have now been described ; and from what I have observed in myself, as well as from other considerations, I cannot help thinking, that every person experiences

periences these three states once a-day, in a greater or less degree, according as their body is more or less irritable.

At night there is certainly formed a febrile state, (as Dr. Cullen mentions) even in the most healthy ; in myself I can very easily perceive it ; and to this I would attribute my uniformly observing my urine paler in the evening than at any other time of the day, except where a diaphoretic had been used, evidently preventing the febrile state. This then is the first state of the urine mentioned above.

The second seems to take place in the night time, especially towards morning. During sleep, there is a relaxation of that febrile state formed in the evening ; and hence one reason of the morning urine being higher coloured than that made at any other time of

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the

the day; this urine likewise more generally deposited the cream-coloured sediment. When I first began these experiments on the urine, I expected to find, according to the general opinion, that the morning urine, as being highest coloured, would also deposit most lithic acid; but the result of repeated experiments convinced me, that this was not the case; so much the contrary sometimes happened, that having kept the morning and mid-day urine of the same day, each 48 hours, I found not above a few particles deposited from the former; while in the latter there was a copious sediment of lithic acid and this notwithstanding that the morning urine was both higher coloured, and in greater quantity.

The mid-day urine forms the third state; this I generally observed of a colour not so dark as the morning urine, nor so light as
that

that of the evening ; but depositing a greater quantity of lithic acid than either. These appearances of the mid-day urine are perfectly explicable on the principles already laid down ; the application is so evident, that it is unnecessary to spend time in showing it.

We must suppose the same diurnal revolution to take place in the skin. In the evening during the febrile state, it will be constricted ; during sleep relaxed ; and in vigorous action during the day-time. I have already mentioned my reasons (Experiment xxi.) for supposing that the acid occasioning the precipitation of lithic acid is only thrown off by this organ, as by the kidney, in proportion to its vigorous action ; hence there will be constantly an accumulation of acid during the night time, to be thrown off, the

the following day by the renewed vigour of the skin and kidneys.

These observations tend to establish a fact of considerable importance, with regard to the pathology of gravel, viz. That it is by the vigorous action of the skin and kidney, that any dangerous accumulation of acid must be guarded against; former observations pointing out, that no abstinence from acescent *ingesta* whatever, is sufficient for this purpose.

Upon the whole, from the foregoing experiments and observations, I would conclude, in the 1st place, That any cause obstructing perspiration produces a greater than ordinary precipitation of lithic acid from the urine. 2^{dly}, That the same precipitation is, *cæteris paribus*, increased by acescent diet, and much diminished by using a large proportion

tion of animal food. *3dly*, That by the inactivity of the skin and kidneys, an accumulation of acid may take place in the system, only to be thrown off, by restoring their proper action. *4thly*, That by the proper use of diaphoretics, we can often entirely prevent the deposition of lithic acid from the urine. *5thly*, That the quantity of lithic acid deposited is by no means in proportion to the height of the colour of the urine. *Lastly*, We must conclude from Mess. Scheele's and Bergman's experiments, as well as from the above observations, that it is the lithic acid which is apt to form insoluble concretions in the urine ; hence the danger of all those circumstances of life, tending to occasion its precipitation, as will be fully seen in the following chapter.

Having now shown what the circumstances are, which tend to produce a deposition

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of

of the lithic acid, I proceed to show, that all the predisposing causes of gravel act, by precipitating the same acid ; and to point out, that all these causes produce the same change on the body ; which change we must consider as *the remote cause of the disease.*

CHAP.

C H A P. IV.



The Application of the foregoing Experiments to determine the Remote Cause of Urinary Gravel.

I COME now to make an application of the foregoing experiments and observations, in order to determine the change induced on the body by the predisposing causes of gravel; or what is properly called *the remote cause of this disease*. From what has been said in the three preceding chapters, I hope I shall be able to explain the action of the different circumstances in life predisposing to gravel; and show that all these produce the same change on the body.

What

What I would look upon, then, as the chief predisposing causes of gravel, are, *1st*, Too great a proportion of solid from the particular formation of the body. *2dly*, Old age. *3dly*, Excessive labour. *4thly*, High living, and the liberal use of fermented liquors. *5thly*, Indolence; and, *6thly*, Too much heat applied to the body in general, or particularly to the kidneys.

In the *first* place, I am to determine how too great a proportion of earthy matter from the particular temperament of the body, should give a predisposition to gravel.

That this really takes place, we see from men being more subject to this disease than women; whose bodies are more lax, and consequently contain a less proportion of earthy matter;—and likewise from this, that wherever there is a greater proportion of
 earthy

earthy matter in the system, the watery part of every secretion must be proportionably less; hence in robust men we find the urine higher coloured than in those, *cæteris paribus*, of a more lax habit of body.

At first sight, we might be apt to attribute entirely to this cause, the fact just now mentioned; but on considering what I have related, it will appear, that there is another cause predisposing to gravel, and one perhaps of greater consequence, which people of this temperament are particularly subject to.

It has already been shown, that any cause obstructing perspiration, throws a greater quantity of acid on the kidneys than ought to pass by that outlet. If we consider, then, for a moment, we shall find several causes rendering people of the above temperament subject to failure in this necessary discharge.

In

In the *first* place, in them the fluids move slower, and consequently will be less disposed to support an excretion, which seems particularly to depend on the general activity of the sanguiferous system. *2dly*, It is well known, that as the human body advances in life, it gradually acquires greater firmness, and that an excretion of the smaller vessels is continually going on; now people who have naturally a greater firmness of body, will experience this the sooner, and in the greater degree. As the skin then is chiefly composed of these small vessels, perspiration must suffer a considerable check by this cause; and thus a great quantity of the acid matter which ought to have passed by the skin, will be thrown on the kidneys. *Lastly*, The same causes affecting the skin also tend to destroy the action of the kidney, thus shutting up the only natural vents given this matter, and unavoidably occasioning an
ac-

accumulation of it in the system, which at last will be forced off by the kidney ; and there precipitating the lithic acid in a greater quantity than is consistent with health ; this will be deposited, and thus lay the foundation for a fit of the gravel, and sometimes for a worse disease.

From these considerations, it appears in what manner an over proportion of earthy matter from the original conformation of the body predisposes to gravel. I am now therefore to consider in what manner old age induces the same disposition.

That old age predisposes to gravel, none will deny ; every day's experience has told us, how subject that period of life is to calculous complaints ; a circumstance which seems in a great measure to depend on what I have already hinted ; for in all temperaments,

ments, as old age advances, perspiration is gradually checked, partly owing to the increasing proportion of the solids, partly to the gradual debility induced on the powers supporting perspiration, and partly perhaps to the long exposure to the air the skin has undergone, which, by its gentle, but long continued friction, must tend to obstruct the minute pores spread over the surface of our bodies.

The same causes affecting the kidney in the former case, act also here ; occasioning a greater accumulation of acid in the system ; and when we consider that the separation of this depends so much on the vigorous action of the skin and kidneys, (as it seems to do from what has been observed), we shall find in that weakness, induced by old age, not perhaps the least of the causes, which

which prevent the proper expulsion of this matter by urine and perspiration.

There yet remains a circumstance often occurring in old age, which predisposes to gravel, in several ways; I mean dyspepsia: but of this I particularly speak afterwards.

The next cause mentioned as predisposing to gravel, is excessive labour, which seems to act by inducing a state similar to old age, before that period has arrived. Van Swieten mentions a person who died of old age between 30 and 40, from the excessive labour he had all his life been engaged in.

The predisposing cause of gravel I am now to consider, is high-living, and the too liberal use of fermented liquors. That this strongly predisposes to gravel, appears evident from the experience of every age; which

has

has shown us the indulgence in such gratifications, severely compensated by the accession of this disease, in the decline of life.

From what has already been said, will appear the manner in which I would explain the action of this cause. In the *first* place, by the repeated application of stimuli, it finally debilitates the system, thus tending to diminish the vigour of circulation, and consequently the action of the skin and kidneys. But it has a farther, and perhaps a more important action ; it particularly debilitates the stomach. The changes induced on the system by the debility of this important part, I am now to consider.

The first change induced on the system by dyspepsia, to be taken notice of, is an increase of that general debility, which I have already mentioned, as arising from the repeated

peated stimuli, applied by high-living, and the liberal use of fermented liquors. The second, that want of action in the skin, which always attends the debility of the stomach. It is needless, after what has been so often said by authors, and particularly Dr. Cullen, to attempt, by any arguments, to prove that close connection, which takes place between the skin and stomach; and from which, must proceed the inactivity of the former in every dyspeptic patient. Thus far then dyspepsia predisposes to gravel, by throwing on the kidney, a great part of that acid which passes by the skin, in a healthy person.

But there is another manner in which dyspepsia acts: I mean by producing acidity in the *primæ viæ*; for, from the observations of others, as well as from the experiments above related, it appears, that by increasing the acid in the *primæ viæ*, we, *ceteris paribus*,

bus, increase the quantity of the lithic acid deposited by the urine.

The circumstance which now falls under consideration is, perhaps, the most powerful of all the predisposing causes of gravel: I mean indolence. The instances of gravel induced by this, are numerous; and many are the different ways in which it predisposes to that disease.

Its action on the body is, in some respects, similar to that of fermented liquors; it weakens the circulation, and induces dyspepsia; by these means, predisposing to gravel, in the way I have already endeavoured to explain. But the manner in which this cause acts, is, perhaps, still more pernicious, while high-living and fermented liquors finally induce debility, by applying repeated stimuli to the system; it induces a more direct

rect debility, particularly of worse consequence on this account ; that while the former causes act, they support perspiration ; but this from the first tends to destroy it ; for, by continuing in indolence the cuticular pores must gradually close, since they are never properly excited to action ; and thus, produce a more inveterate malady than could arise from the mere debility of the skin.

I come now to consider the last of the predisposing causes of gravel, which has been mentioned. The effects of much heat unassisted by any of the other predisposing causes, might probably be insufficient to give any considerable predisposition to the gravel ; or might even, by supporting perspiration, tend to prevent it. We see the gravel more rare in warm climates. It is, when it accompanies some of the more powerful, and particularly the last mention-

ed predisposing cause of gravel, especially in the colder climates, where perspiration is more apt to fail ; that it seems by weakening the action of the skin and kidney, to conduce to the accumulation of the acid matter in the body.

Heat applied to the kidneys, has ever been looked upon as prejudicial, and if we consider how much the expulsion of this acid depends on the vigorous action of the kidneys, we shall not be surpris'd, if any cause tending to debilitate this, prove pernicious to the system.

There still remains one circumstance looked upon by all, as predisposing to gravel, viz. the presence of the gout. I have not mentioned it, however, as such, considering this effect, produced by the gout, inducing
some

some of the predisposing causes already mentioned.

There is no reason for supposing these two diseases essentially connected. *1st*, Because we do not see the gravel accompanied by the gout, when the former is the primary disease. *2dly*, We may often account otherwise for their concurrence, since the same causes applied, through life, tend to induce both diseases. *3dly*, We often see the gravel induced by other diseases and accidents; when they are accompanied with the circumstances I am soon to consider as occurring in the gout; for, with equal reason, might we reckon a fracture essentially connected with the gravel, since it has often induced that disease. *4thly*, We do not see the gravel induced by the gout, till it has been present some time; and the predisposing

ing causes of gravel I am to mention, have been remarkably applied.

The first of these is indolence, which ever accompanies severe cases of gout, and predisposes to gravel, as has been shewn ; the second is, that great degree of dyspepsia, which at all times, by diminishing the action of the skin, and producing acidity in the *primæ viæ*, aggravates the effects of indolence, and strongly predisposes to gravel. The heat also, often applied to the region of the kidneys, from the patient lying on his back, and the situation of the kidney lower than the bladder ; and hence, the urine perhaps more than it ought to be retained in its pelvis, must tend to weaken the action of that organ ; and thus to produce an accumulation of acid matter in the system.

Now

Now, if we consider what I have already mentioned, that the manner of life inducing the gout, also predisposes to gravel; we shall not be surprised, that the above causes often induce this disease in gouty patients, although there be no essential connection between the two complaints. Not that I would deny that the gravel and gout proceed from the same noxious matter, existing in an over-proportion in the system; but that I think we cannot be certain of this, till we know more of the nature of the gout. Be this however as it will, it still appears that the diseases are connected in the manner which has been explained.

Hence we see, that the gravel will alternate with the paroxysms of the gout; for, during these, the perspiration is better supported; the stomach less affected with dyspepsia, and every part of the system in great-

er activity, than in the atonic state which intervenes between the paroxysms, where the want of vigour in the skin and kidney, (which last is surely much hurt in the preceding paroxysm of the gout, by the causes which have been mentioned), together with the acidity of the *primæ viæ*, must occasion an accumulation of acid in the system, soon to be forced off by the kidney.

It has now been shewn, that all the predisposing causes of gravel induce the same change on the body, viz. the inactivity of the skin and kidneys, hence an over-proportion of acid matter in the system; and a deposition of lithic acid from the urine, so soon as it passes by the kidneys. This inactivity of the skin and kidneys then, we must reckon, *the remote cause of gravel.*

It

It only remains now, to mention certain circumstances rendering it probable, that an accumulation of acid has taken place, and the means for clearing the system of it, before it be forced by the kidney.

C H A P. V.



*Of the Circumstances rendering it probable,
that the remote Cause of Gravel is present,
and the Means for removing it.*

IN the *first* place then, as an over-proportion of this matter is gradually accumulating in the system, by the inactivity of the skin and kidneys; the gravel must be a periodical disease; and as the inactivity of these organs is gradually increasing, the fits of the gravel must by degrees become more frequent. This is confirmed by experience; hence people subject to the gravel, know when to expect its return; and consequently, when there is an accumulation of acid in the system. *2dly*, When the gout has continued

continued for some time, and the paroxysms have been frequent, experience as well as the above considerations assure us, that an accumulation of acid has taken place. *3dly*, When Hypochondriasis occurs in old age, after the predisposing causes of gravel have been applied, there seems good reason for supposing an over-proportion of acid in the system. *Lastly*, every old person of the temperament predisposing to gravel, particularly, if a male, and more so, if this disease has been hereditary in the family, may dread an accumulation of that noxious matter, sooner or later, about to make its appearance by inducing a fit of the gravel.

When it appears, that the remote cause of gravel has taken place, we must use every means in our power for correcting this morbid condition of the body, and preventing it from relapsing into the same state.

In attempting this, there are four indications
1st, Strengthening and assisting the digestive
 organs. *2^{dly}*, Avoiding such *ingestæ* as in-
 crease the quantity of the matter we endea-
 vour to expel. *3^{dly}*, Using such as have
 an opposite tendency. *4^{thly}*, Throwing out
 this matter by every means in our power.

With regard to the first indication, it
 forms a chief part of the treatment in gravel,
 and is fully considered in the last part of
 this paper: I am therefore to speak here of
 the other three.

It has already been shown, that acedent
 diet tends to precipitate the lithic acid from
 the urine in great abundance; and on the
 contrary, that alkalescent food diminish-
 es its quantity. The patient ought certainly
 then to live as much on this as possible, of
 which fish of all kinds seem the best, as, in
 general,

general, easily digested, and producing a more alkalescent state of the body than any other diet. To this it would certainly be a good addition, as far as the state of the body will permit ; to take, for a certain time, such alkaline matters, as have been found best fitted for the purpose, such as lime-water, aerated alkaline-water, &c.

I come now to the last indication which I must consider at greater length, viz. throwing the acid matter out of the body.

Diluents seem to have been employed with this view ; but from what was formerly said, deduced from repeated experience. Diluents seem to be of less service, than at first sight we might be inclined to suppose.

Diuretics appear medicines well suited to this indication, and have been accidentally employed

employed in the disease ; but a diuretic which has proved successful, has rather been regarded as a specific, than a medicine, the manner of whose action was understood ; and consequently, pointing out a class, in which different individuals might suit different constitutions.

But all diuretics often fail of exciting the action of the kidney ; and it may prove fortunate, that nature has given another outlet to this matter, and one whose action we have it more in our power to command : I mean the skin. The experiments which have been made with diaphoretics, and sudorifics, shew, that the noxious acid, whether derived from acid *ingesta*, or generated in the body, is perspirable ; and that, by increasing the action of the skin, we can so entirely free the system of it, that little or none is secreted by the kidney.

If

If this were only a part of the groſſer perſpiration, it would be attended with much inconvenience to throw it out of the body in this way. But the experiments made with diaphoretics ſhew, that this is by no means the caſe, and that the ſyſtem may be moſt effectually relieved from its acid matter, merely by increaſing inſenſible perſpiration, without the leaſt inconvenience to the patient ; for, as I have already mentioned, I have always found tartar emetic moſt efficacious, when it produced no nauſea ; and obſerved, that this medicine produced a greater effect on the urine, and a more laſting one, than Dover's powder, although inducing a copious ſweat,

Hence it appears probable, that we may find in the diſeaſe I now treat of, another uſe for this valuable medicine, no leſs important

portant than those to which it has been already applied.

There is another remedy, which, from what has been said, (Experiments xxii. and xxiii.) promises to be of much advantage in this disease. I mean mercury; this medicine is apt to promote the action of both the skin and kidney, so that it is admirably suited for the last of the indications above mentioned. And the more so, as it certainly produces its effects, as almost any medicine we are acquainted with.

The only objection which could be urged against giving mercury in this case is, that being a medicine, the use of which tends to debilitate the system considerably, it might be improper in a disease which generally depends much on debility, and in circumstances

ces

ges where it would be necessary to continue its use for a considerable time.

To the first part, however, of the objection, it might be answered, that mercury given in small quantity, is not apt to debilitate the body in any great degree. Dr. Gregory, professor of the Practice of Medicine in the University of Edinburgh, observes, that he has often seen people under a gentle and well conducted course of mercury, recover both flesh, and a healthy appearance. When given to a moderate extent, it generally produces the effects here desired, in the fullest manner. Hence the good effects of mercury in dropfy, even where this proceeds from debility. It is only given in such a manner, that it could be used; and has all the appearance of proving useful in gravel. The form of ointment applied to the skin, as being that in which mercury is least apt to affect

fect the stomach, would probably prove the most convenient manner of giving it. (See Experiment xxii.)

With regard to the latter part of the objection, were its exhibition well timed, it would not be necessary to continue it very long. The time most proper for this is very evident, viz. a little before the paroxysm is expected; and to be continued till the ordinary time of its appearance is past, in order gradually to expel by the skin and kidneys that dangerous accumulation of acid, which seems at this time to take place, and to occasion the periodical return of the disease.

As exercise excites both the action of the skin and kidney, a proper degree of this will always be a good addition to what has been proposed; but in no case, perhaps, can we trust entirely to it, or expect, that the body,
already

already weakened, will, by its own powers alone, restore the vigour of organs, so long habituated to inactivity.

Upon the whole, we answer the last indication by exciting the action of the skin and kidneys. The certainty with which we can increase that of the former, allows me to hope, that, by the means here proposed, we may sometimes prevent a disease, one of the most distressing the human body is subject to.



P A R T II.

OF DYSPEPSIA.

C H A P. I.

INTRODUCTION.

THE disease I am now to treat of, is more neglected perhaps by physicians than it deserves to be. Considered in itself, indeed, it is not attended with any immediate danger, but often with extreme distress to the sufferer; considered in its consequences and connexions, we shall find it of no small importance. It is surely worth while, then, attempting to inquire more particularly into the proximate cause of this disease, than

has been done ; and to see, if, by doing so, we can throw any light on the treatment of itself, or of those more important diseases with which it is connected. This, then, I shall do, dividing what I have to say on dyspepsia, into An Inquiry concerning the proximate Cause of it, and Some Remarks on its Cure.

Before I proceed to this, however, it is necessary to determine, from the numerous experiments and observations, which have been made by Spallanzani and others, on the subject, what we are to esteem the efficient cause of digestion in the human body.

We see, in many animals, such a structure as seems fitted to excite an incipient fermentation, previous to the digestion of their food. Spallanzani himself found, that food underwent fermentation in the crop of gallinaceous

ous fowls. In ruminant animals, the food lies long before it can be digested; which only takes place in the last stomach. Many animals, particularly insects, choose food far gone in fermentation.

Thus, there can be no doubt of the food entering the stomach of all these different animals, in a state of greater or less fermentation. To this I may add, that Spallanzani constantly found an acid present at the beginning of digestion, when the animal had taken vegetable food: and this he candidly owns, although seemingly contrary to his own theory of digestion.

If this fermentation, then, be not favourable to digestion, it must retard it; for the same person has shown, that all the sensible qualities given food by fermentation, must be destroyed by the gastric liquor, before it

can be digested. Was nature then obliged needlessly to multiply her work? Could she not have given the liquor of the crop of gallinaceous fowls an antiseptic power, as she has the gastric liquor? Or why do so many animals choose fermenting food, if a certain degree of fermentation be not favourable to digestion?

These are arguments drawn from other animals; but there are also arguments drawn from man himself in support of this opinion.

When we consider the food reduced to a pulp, by the action of the teeth, and mixed with the saliva, (a fluid found, by Spallanzani, Macbride and Pringle, to promote fermentation), sent down in mouthfuls, and pressed by the act of deglutition; so that the gastric liquor cannot immediately pervade it; when we consider the warmth of the human stomach; and, lastly, that a mixture of food is
equally

equally favourable to fermentation and digestion in a sound stomach ; we cannot do otherwise than conclude, that some degree of fermentation must take place previous to digestion ; and that it promotes this operation.

Thus, there seem many arguments for an incipient fermentation, previous to the digestion of our aliment ; and the experiments of Spallanzani do not seem to deny it ; as he always judged of its presence by intestine motion, and extrication of air ; appearances which only become sensible in more vigorous fermentation.

Yet Spallanzani's experiments prove beyond reply, that fermentation is not the efficient cause of digestion ; we must therefore suppose it subservient to it, and that the truth lies between the two opposite sides of this disputed question.

With

With regard to the use fermentation serves, I would offer the following conjecture, viz. That an incipient fermentation, in a manner similar to mastication, by minutely separating the more solid parts of our food, renders it more pervadeable by the gastric liquor; and consequently more easily digested. Hence one use of the crop in gallinaceous birds; where there is no mastication, and the food of very difficult digestion.

That a certain degree of fermentation renders our food more digestible, appears from many observations. Dr Withers, in his dissertation on chronic weakness, says, “ animal
“ food should be kept some time before it is
“ used, that, by having undergone a slight de-
“ gree of fermentation, it may be sufficiently
“ tender, and easy of digestion.”

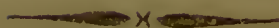
Induced by such considerations, we cannot help concluding, that a slight degree of
fermen-

fermentation, previous to digestion, is favourable to that process. But we must still look upon the gastric liquor as the instrument of digestion, while mastication, trituration, and fermentation are beneficial only by facilitating its action.

Having shown, in a few words, what is to be considered as the efficient cause of digestion, we are now better prepared for speaking of the circumstances which impede it.

C H A P.

C H A P. II.

*Of the Proximate Cause of Dyspepsia.*

IT is surprising, while so many experiments have been made, in order to determine the efficient cause of digestion, that no person has endeavoured to investigate the proximate cause of its failure.

It has, however, been the opinion of authors, that dyspepsia is owing to one of three causes ; either to a diminution of the muscular action of the stomach, or to a vitiated state of the gastric liquor, so that instead of digesting the aliment properly, it seems to promote a morbid fermentation, tending to
obstruct

obstruct the necessary change of our food ; which, in its healthy state, it is the chief means of effecting ; or, lastly, to a deficiency of the same liquor.

Dr. Cullen seems to have been of the first opinion, as he mentions in paragraph 1193 ; his words are : “ That there is a distinct disease, attended always with the greater part of the above symptoms, is rendered very probable by this, that all these several symptoms may arise from one and the same cause, that is, from an imbecillity, loss of tone, and weaker action of the muscular fibres of the stomach ; and I conclude, therefore, that this imbecillity may be considered as the proximate cause of the disease I am to treat of under the name of Dyspepsia.”

Dr.

Dr. Cullen, however, mentions in paragraphs 1195 and 1196, that a change in the quantity or quality of the gastric liquor, may constitute another proximate cause of dyspepsia ; and surely, since the experiments of Spallanzani and Dr Stevens, we cannot suppose that a weakness in the muscular action of the stomach induces dyspepsia in any manner, but by changing the gastric liquor ; and therefore that it never can be reckoned the proximate cause of this disease. It remains, then, to consider in what manner the gastric liquor is changed.

With regard to the second hypothesis mentioned, many circumstances render it probable, that this is not the proximate cause of dyspepsia. But as my reasons for forming this opinion, will sufficiently appear, from what I shall say of the last mentioned hypothesis ; to avoid needless repetition, I shall
 enter

enter immediately upon the consideration of this ; where I shall, in the 1st place, show, in what manner it explains the operation of the occasional causes of dyspepsia. 2^{dly}, The symptoms occurring in dyspepsia ; and, lastly, the methods practised for alleviating or curing this disease ; and, as I go along, shall point out, that all of these are less explicable by the second supposition, than by that I have undertaken to defend.

With regard to the occasional causes of dyspepsia, I shall be very short ; it is needless to enter into an explanation of each of them. They are very numerous, as related by Dr. Cullen in his first lines, and Dr. Withers, in his treatise on chronic weakness ; they are all such as tend to weaken the stomach directly, or the system in general ; and it is surely more conformable to what we know of the animal œconomy, to suppose, that

that causes acting in this way, should rather produce a deficiency of the gastric liquor, than change its properties in so eminent a degree. But, independent of this consideration, there are some of the occasional causes of dyspepsia particularly explicable on the hypothesis I defend ; for example, narcotics and indolence, which are well known to lessen every secretion.

I am now to show in what manner the same hypothesis accounts for the symptoms occurring in this disease.

With respect to the production of acidity, and its consequences, as they are easily explicable on either supposition, I pass them over. There is one thing on this head, however, I have always observed, which tends to confirm my opinion concerning the proximate cause of dyspepsia. It is, that although a
dyspeptic

dyspeptic cannot digest an ordinary quantity of food, without morbid symptoms, yet will he digest a smaller perfectly (I do not talk of the extreme case of dyspepsia, where there is certainly no digestion at all). I have also frequently observed in myself, (for I have been much troubled with dyspepsia), that if I fasted several hours longer than usual, the fermentation in my stomach was corrected, and the food perfectly digested; must we not suppose, then, that this was owing to the gastric liquor which had flowed in during this time; and that, had this quantity of liquor been supplied soon enough, the food would have been digested without any dyspeptic symptoms; and consequently that these were owing to the failure, and not to any depravation of the gastric liquor.

Before I proceed farther in explaining, by this hypothesis, the symptoms of dyspepsia,
I shall

I shall relate a severe fit of it I suffered some time ago ; as the inferences we must draw from this, tend to confirm the opinion I have undertaken to support.

I was seized above a year ago, after a long confinement, with great anxiety, weakness, and complete anorexia, which lasted near four days, accompanied with considerable thirst, and failure of saliva. During the second night of the above, having sucked an orange, in order to remove the disagreeable dryness occasioned by the want of the saliva ; next day I felt nausea, and oppression referred to the stomach ; which induced me to evacuate it, by irritating the fauces, about eight hours after I had taken the orange juice, I was much surprised to find it, after that stay in the stomach, unaltered and unmixed with any other substance.

The

The inferences I would draw from this, are the following.

1st, That my dyspepsia was owing to a deficiency of gastric liquor ; for, it was evident, that there was nothing in my stomach at that time ; but the orange juice which had undergone no change, because there was no gastric liquor present to digest it.

2^{dly}, That nausea, in such circumstances, arises from the most grateful food.

3^{dly}, That anorexia seems to proceed from the complete failure of the gastric liquor ; and that this sensation is a wise provision of nature, which prevents us from eating at a time when no digestion could go on ; by which we should produce repeated vomitings, without receiving any nourishment.

K

Although

Although these inferences appear well founded, yet, in order to confirm them, it was necessary, as far as possible, to reduce them to experiment.

From the above it would appear, at first sight, that, by emptying the stomach of its gastric liquor, we might, at will, produce anorexia; from what has been said, as well as from what I am going to mention, I believe we might; but it is a very difficult matter to empty the stomach entirely of its gastric liquor. *1st*, Because it is difficult to empty it entirely of any of its contents. *2^{dly}*, Because the very act of vomiting, by the strong stimulus applied to the stomach, excites it to pour out a fresh quantity of its liquor; but that anorexia can be nearly produced, and the sensation of hunger almost entirely taken away, by freeing the stomach
of

of the gastric juice, appears from the following experiment.

Eating nothing after dinner, nor drinking any thing but pure water; next morning I still increased my appetite by walking. On returning home, I felt exceedingly hungry, having eat nothing for above 17 hours; instead of taking breakfast, I got a quantity of luke-warm water, and by means of this, repeatedly excited vomiting.

The water came up quite clear, and only mixed with a ropy transparent fluid, such as the gastric liquor is described by Spallanzani, or as I have myself procured from the stomach of a crow.

This plainly indicated, that there had been nothing in my stomach but the gastric

liquor; which was mixed with the water, and in that state, without sensible taste, smell, or colour.

After I had undergone this operation, I found every sensation of hunger removed, and rather a disgust for food produced, which I sensibly felt on seeing others eat. I sat down, however, to breakfast, but found myself satisfied, even to sickness, after eating half my usual quantity; this continued for several hours, accompanied with oppression referred to the stomach, and anorexia.

Although I could not perhaps, by these means, produce complete anorexia, for the reasons already given, yet any person, I am persuaded, can produce it so far, as to require a very small quantity of food indeed, to supply every craving for it.

From

From these two cases, then, I cannot help thinking, that the sensation of hunger depends on the presence of the gastric liquor, unmixed with any alimentary matter which alone can divert the powerful action of this fluid from the stomach itself; and that the deficiency of the same fluid is the proximate cause of anorexia:

This hint might probably be of service to those who are in such situations, that they cannot procure enough of food otherwise to allay their hunger: but it is never to be attempted except in extremities; for by repeated vomiting the stomach must be materially hurt, and by the privation of the principal instrument of digestion, the body soon weakened to such a degree, that death would certainly be the consequence of continuing the practice.

Might it not be useful in some cases of dangerous, and very urgent plethora, where it is often impracticable to persuade the patient to resist the calls of hunger?

From what has been said, it would appear, that the presence of the gastric liquor in the stomach, without such substances as are fit for combining with it, and rendering it inert, is a principal cause of that death which is occasioned by hunger; for, we have seen anorexia produced by the evacuation of the gastric liquor; and every one knows, how long a person, labouring under anorexia, will live without aliment.

The speedy acidity which took place in this experiment, is very remarkable. Although my stomach was perfectly free from every fermenting substance, when I went to breakfast, as was evident from the state
of

of what I threw up ; yet, the food I had taken, (bread and milk), acquired acidity in a quarter of an hour, as I found by acid eructations. Was not this artificially producing dyspepsia, by lessening the quantity of the gastric liquor ? For, after this meal, although a very spare one, I felt oppression referred to the stomach, anorexia accompanied with nausea, and acid eructations.

The application of the hypothesis I defend, to explain all the circumstances related, is evident ; if we attempt to do this by the other, we shall find it impossible.

Finally, the thirst, bound belly, and failure of saliva, so often occurring in dyspepsia, support the same opinion.

Having shewn the manner in which I would explain the occasional causes and
 symptoms

symptoms of dyspepsia: I am now to attempt an explanation of the manner in which those remedies act, which are found to alleviate, or cure this disease.

All stimulants relieve the symptoms of dyspepsia. Are we then to believe, that these have indiscriminately a property of correcting the vitiated state of the gastric liquor? Do not they act in this, as in other cases, by stimulating to a more copious secretion? And how can we suppose the dyspeptic symptoms relieved by an increased secretion of the gastric liquor in a vitiated state? But stimulants have another action when too often repeated; they, by degrees, increase the tendency to dyspepsia. This part of their action, likewise, is explicable by the same hypothesis; for the debility induced on the stomach by the too frequent application of stimulants must ever tend to lessen its secretion.

It

It is also evident, in what manner, by the same supposition, we may explain the operation of the other remedies used in this disease. Internal tonics strengthen for a time, the system, enabling the stomach to supply a quantity of gastric liquor sufficient for the purposes of digestion ; but these, like the former, either by applying an unnatural stimulus to the body, or by a narcotic power, at length prove hurtful, on the principles already explained.

There still remain to be considered, two great remedies, I mean Exercise and Cold-bathing ; of their action, however, I say nothing, as this is so well known, and so easily explicable on the present hypothesis.

From considering what has been said, it will be sufficiently evident, I hope, that the
 opinion

opinion I defend is well founded, and capable of giving the pathology of dyspepsia.

Having finished what I had to say on the proximate cause of this disease, I proceed to speak of its cure.

CHAP.

C H A P. III.

*Of the Treatment of Dyspepsia.*

I SHALL not enter fully into this part of the subject ; as that would be only repeating what has so often been said already ; but confine myself to some remarks which have occurred to me, either from my own observation, or that of others, in addition to what authors have said on the cure of this disease.

From what experience has often taught me, as well as from the hypothesis which I
have

have considered as the proximate cause of dyspepsia, I am persuaded, that vomiting, which is generally the first remedy here employed, is for the most part hurtful. I have frequently used it, and always found the same necessity for it next day ; unless I had taken other means for preventing the return of my complaints.

The reason of this is evident ; for, while we vomit, we evacuate the stomach, indeed, of its morbid contents, but at the same time, along with these, of its gastric liquor ; so that, by throwing in food after vomiting, the sourness and troublesome symptoms soon return, because there is no gastric liquor in the stomach to check fermentation, and digest the food. But if the patient, after vomiting, should fast some time, till the gastric liquor flow into the stomach in sufficient quantity ; and only take at once a small
quan-

quantity of food ; the dyspeptic symptoms will not be so apt to recur.

In Dr Gregory's *Confectus Medicinæ* he says, vol. i. p. 344. “ Neque profecto aut
 “ qualitas aut mixtura ciborum, (quamvis
 “ hæc sane haud parum aliquando nocent
 “ sicubi totam massam nimis cito fermentescere facit) helluoni tantum nocent
 “ quantum ingens copia quam ingurgitat.”

As every author is of opinion, that, by frequent vomiting, the stomach is considerably weakened, this ought to be avoided as much as possible ; and in most cases, it can be so entirely : for I have often seen dyspepsia removed by fasting, without any vomiting at all : and almost every dyspeptic patient will find, that, although he cannot digest his usual quantity of food, yet he
 can

can a less, without any morbid symptom. Hence he ought to use the most nourishing, and at the same time the most digestible food, in such quantity only, as experience teaches him, his stomach can easily bear : by which means he will be enabled to avoid the use of many medicines, which, although affording present relief, tend to confirm this disease ; in which, we may almost lay it down as a rule, that the more internal remedies we employ, the more obstinate the complaint becomes.

In young people labouring under dyspepsia, this disease is generally accompanied with a morbid irritability, which seems often to give rise to it. In such indispensibly, and indeed in all dyspeptics, those remedies must always be employed, which act on the system in general, and which are by nature so adapted to the human body, that they give it

it the proper stimulus without any subsequent injury ; with this caution, however, that they be not applied when the body is too weak, or at any time in too great a degree ; for by such an error, they always exhaust the strength, and increase every complaint, which they are intended to alleviate or cure.

These remedies are, in the *first* place, Exercise. *2dly*, Cold-bathing ; of the former I shall speak afterwards ; of the latter I say nothing ; every one is acquainted with the propriety and the methods of employing it.

There is one valuable remedy of this kind, however, which has been less attended to : it is to be regretted, that it can so seldom be used with propriety ; and that when it is, it is often carried to such excess, that it proves
more

more frequently the cause, than the cure of dyspepsia.

It is certainly on this account, that it has not been openly recommended. I have however seen the moderate use of it, when it had not formerly been employed, attended with such sudden, and remarkably good effects, where almost every other remedy had failed, not excepting the proper use of exercise and cold-bathing, that I cannot help recommending it to the attention of those, who are qualified to know when it may be proper, in circumstances where it can be made use of, to employ it. The remedy I allude to is the *usus modicus Veneris*. Many have remarked its good effects in chlorosis, a disease much akin to the one I treat of; which is often caused by, or attended with debility of the genitals, even in the male. We may say, I believe, of this remedy,

medy, what we can hardly do of any other recommended by physicians, that it only requires to be less agreeable to the patient, to answer every intention for which it is designed.

There is still one thing to be considered, and which is always of the utmost consequence; I mean the proper regulation of sleep,

Every dyspeptic patient will soon discover, that too much sleep is equally bad with too little. For, as the proper degree of exercise is that which applies the necessary stimulus to the system, while a greater degree overpowers it; so there is a proper proportion of sleep, which refreshes the body after its daily exertions; while a greater, by diminishing the application of those stimuli, necessary for preserving the body in a healthy state, con-

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stantly.

stantly tends to relax it. This is not all ; for the time of the day spent in sleep seems to be of equal consequence to weakly people with its duration.

Nor is this fact inexplicable, if we consider the diurnal revolution to which the human body is subjected ; and which becomes the more sensible, the more we are weakened by disease. In the evening, there is surely formed, more or less of a febrile state, even in the most healthy ; in those of an irritable habit of body, and particularly in the young, it is very discernible. As the continuance of such a state tends in a great degree to debilitate the body, we must avoid it as much as possible ; there is but one effectual method of fulfilling this indication, and which is universally pointed out by nature ; I mean repose. Hence, dyspeptic patients find much advantage in going early to bed ;
for

for whatever tends to exhaust the strength, increases, in a remarkable degree, the symptoms of this disease.

The same considerations explain the fact formerly mentioned, viz. the harm done by indulging in sleep for too long a time ; for after the relaxation of the febrile state, and after the body is sufficiently refreshed, any longer continuance of sleep tends unavoidably to weaken and relax it.

If a person then has a mind to protract the time allotted by nature for repose, he ought to do so, rather by going early to bed than lying long in the morning.

It is difficult to point out exactly any rule with regard to the duration of sleep and the time of the day most proper for indulging it, which might be universally applicable.

The result of several years' observation, during which time I was much troubled with dyspepsia, has convinced me, that I always enjoyed best health, when I slept about 7 hours and a half, and when I went to bed between ten and eleven in the evening, having made a full trial of going to bed earlier and later, and of sleeping a longer or shorter time.

Of such consequence is the regulation of sleep in dyspepsia, that I have known a person labouring under this disease, use every remedy in vain, while he indulged too much in it, and by properly regulating this alone, having intermitted all other remedies, become free of every morbid symptom. I have also observed, that the weaker I was, and of consequence, the more evidently the febrile state was formed, the more I suffered by any want or excess of sleep; a circumstance

stance which sufficiently supports the explanation I have offered.

From what has been said it results, that there is also a certain time of the day most proper for exercise. Hence dyspeptics find walking in the morning among the best remedies for their complaints. Much of the good effects of this, however, must be attributed to their getting out of bed at a proper time, though that they are not entirely to be accounted for in this way, is taught us by experience.

It may appear to some, that I have been too prolix on this part of my subject. But the great advantage derived from a proper regulation of sleep and exercise, not only in dyspepsia, but in every chronic disease accompanied with weakness, affords the best

apology ; and as I would strike out as far as possible all internal remedies, used in this complaint I can never dwell too long on that, which, with a proper attention to diet, already taken notice of, forms the plan that, in most cases, I would propose in their stead ; since experience, on which alone we must depend, has proved it to be effectual.

I have said nothing of the state of the mind in this disease ; concerning which, many useful regulations are laid down ; as these have often been taken notice of elsewhere. The great rule is, to keep the mind constantly engaged, but never to fatigue or overstretch it.

The general view in this treatment, however, is more adapted to the young than the old : In whom, and indeed, in all labouring under severe dyspepsia, and hence much debility

bility, digestion is so weakened, that the body can hardly be supported, far less strengthened to such a degree, as is necessary for employing the remedies mentioned above; and which we may consider as the treatment for a radical cure. On this account, we must use some artificial means of supporting, for the present, the digestive powers; and it remains to be determined, in what manner this may be most safely and effectually done.

Stimulants and internal tonics, have been universally used with this intention. Physicians, however, have always complained, that while, by these, they relieved the urgent symptoms of the disease, they unavoidably increased the tendency to it. It would, therefore, be of much consequence to dyspeptic patients, if a remedy could be found, which would relieve the symptoms of their
disease

disease, without tending still farther to increase the debility of the stomach. Such a remedy, I cannot help thinking, is pointed out by what is said in the last chapter concerning the proximate cause.

We have seen all the occasional causes of this disease, lessening the quantity of the gastric liquor : and every remedy found of service in it, increasing the secretion of that fluid ; hence, instead of preternaturally stimulating the stomach, and thus finally increasing its morbid affections, were we to introduce into it, the gastric liquor of other animals, it appears probable, that the dyspeptic symptoms might, in this manner, be relieved, and the body, by a greater supply of nourishment so strengthened, that by the method formerly taken notice of, a radical cure might be accomplished ; which would the more readily happen on this
account

account, that the patient had not been in the custom of using stimulants, and internal tonics.

There are also other uses to which the gastric liquor of brute animals might be applied; particularly in such cases of *dysphagia*, or vomiting, where it is necessary to nourish *per anum*. The little nourishment which is supplied in this way, is most probably owing to the want of that change which the gastric liquor produces on our aliment, before it reaches the intestines; if the food however, were digested with a gentle heat, for some time, previous to using it, with a proper quantity of the gastric liquor of some brute animal, living on food similar to that of man; it is probable, that the chief circumstance would be removed, which prevents us at present from nourishing patients for any considerable time in this way.

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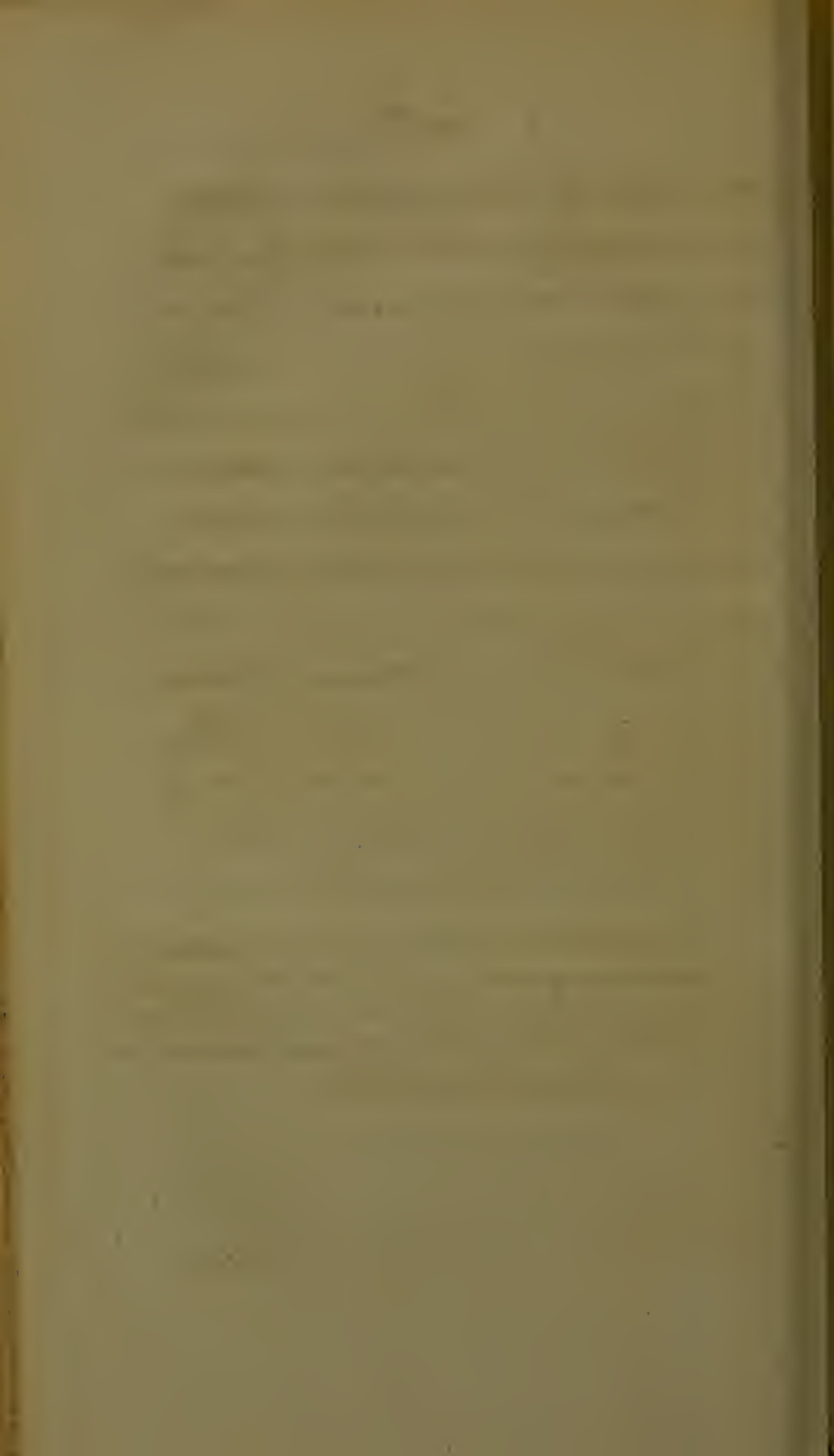
Of the medicines calculated to relieve particular symptoms in dyspepsia, such as absorbents, antispasmodics, demulcents, I say nothing; their effects are transitory, and of little consequence, except that many of them may certainly prove hurtful if used too frequently. The use of these would always be much, and in many cases entirely superseded, by the plan of treatment which I have, from repeated experience, ventured to propose.

I have now finished what I intended doing in this treatise: I have been full on dyspepsia because it appears, that our treatment of this, will ever prove a principal part of the practice in urinary gravel. Whether, by promoting the action of the skin and kidney, in the manner above directed, we shall be enabled to relieve a gravelly disposition, experience must determine: but every age has

has told us, that living in such a manner, as supports their necessary excretions, and at the same time tends to preserve the vigour of the digestive organs, is a method of avoiding a disease which so often makes the decline of life almost intolerable. This can only be done, by steadily observing sobriety, and using that degree of exercise for which nature has designed us.

Printed by A. SHIRREFS, }
Campbell's Close, Cowgate. }

F I N I S.



ERRATA.

PAGE 14.	LINE 8.	<i>read</i> care <i>for</i> cure.
— 4.	— 9.	— Scheele <i>for</i> Sheele.
— 10.	— 11.	— iv. <i>for</i> vj.
— 45.	— 18.	— , <i>for</i> ;
— 67.	— 1.	— mercurial ointment <i>for</i> mer- cury.
— 74. last	—	— not <i>for</i> no.
— 78.	— 7.	— Berthollet <i>for</i> Bertholet.
— 87.	— 4.	— acetous <i>for</i> accetous.
— 106.	— 8.	— accretion <i>for</i> excretion.
— 118.	— 5.	— paroxysm <i>for</i> paroxifm.
— 123.	— 5.	— , <i>for</i> ; & l. 14. r. , <i>for</i> .
— 145.	— 6.	— , <i>for</i> ;

Note omitted in page 88.

If we allow the cream-coloured sediment to grow dry by spontaneous evaporation, it exhibits small white semipellucid crystals ; a circumstance first shown me by Mr. Ruffel, a gentleman who has paid some attention to this subject, and which I have since frequently observed.

